TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

EDINBURGH.

VOL. XXIII.

EDINBURGH:
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THE KEITH, BRISBANE, AND NEILL PRIZES.

The above Prizes will be awarded by the Council in the following manner:—

I. KEITH PRIZE.

The KEITH PRIZE, consisting of a Gold Medal and from £40 to £50 in Money, will be awarded in the Session 1865–66, for the "best communication on a scientific subject, communicated, in the first instance, to the Royal Society during the Sessions 1863–64 and 1864–65." Preference will be given to a paper containing a discovery.

II. MAKDOUGALL BRISBANE PRIZE.

This Prize is to be awarded biennially by the Council of the Royal Society of Edinburgh to such person, for such purposes, for such objects, and in such manner as shall appear to them the most conducive to the promotion of the interests of science; with the *proviso* that the Council shall not be compelled to award the Prize unless there shall be some individual engaged in scientific pursuit, or some paper written on a scientific subject, or some discovery in science made during the biennial period, of sufficient merit or importance in the opinion of the Council to be entitled to the Prize.

1. The Prize, consisting of a Gold Medal and a sum of Money, will be awarded at the commencement of the Session 1864–65, for an Essay having reference to any branch of scientific inquiry, whether Material or Mental.

2. Competing Essays to be addressed to the Secretary of the Society.

3. The competition is open to all men of science.

4. The Essays may be either anonymous or otherwise. In the former case, they must be distinguished by mottoes, with corresponding sealed billets superscribed with the same motto, and containing the name of the Author.

5. The Council impose no restriction as to the length of the Essays, which may be, at the discretion of the Council, read at the Ordinary Meetings of the Society. They wish also to leave the property and free disposal of the manuscripts to the Authors; a copy, however, being deposited in the Archives of the Society, unless the Paper shall be published in the Transactions.

6. In awarding the Prize, the Council will also take into consideration any scientific papers presented to the Society during the Sessions 1862–63 and 1863–64, whether they may have been given in with a view to the Prize or not.

The next Biennial Prize will be awarded at the commencement of Session 1866–67. Competing Essays to be sent to the Secretary of the Society on or before 1st June 1866.

III. NEILL PRIZE.

The Council of the Royal Society of Edinburgh having received the bequest of the late Dr PATRICK NEILL of the sum of £500, for the purpose of "the interest thereof being applied in furnishing a Medal or other reward every second or third year to any distinguished Scottish Naturalist, according as such Medal or reward shall be voted by the Council of the said Society," hereby intimate,

1. The NEILL PRIZE, consisting of a Gold Medal and a sum of Money, will be awarded at the commencement of the Session 1865–66.

2. The Prize will be given for a Paper of distinguished merit, on a subject of Natural History, by a Scottish Naturalist, which shall have been presented to the Society during the three years preceding the 1st May 1865,—or failing presentation of a Paper sufficiently meritorious, it will be awarded for a work or publication by some distinguished Scottish Naturalist, on some branch of Natural History, bearing date within five years of the time of award.

AWARDS OF THE KEITH, MAKDOUGALL BRISBANE, AND NEILL PRIZES, SINCE 1859.

(*Continued from Transactions, Vol. XXII., page vii.*)

AWARD OF THE KEITH PRIZE.

17TH BIENNIAL PERIOD, 1859–61. JOHN ALLAN BROUN, Esq., F.R.S., Director of the Trevandrum Observatory, for his Papers on the Horizontal Force of the Earth's Magnetism, on the Correction of the Bifilar Magnetometer, and on Terrestrial Magnetism generally.

18TH Do. 1861–63. Professor WILLIAM THOMSON, of the University of Glasgow, for his Communication "On some Kinematical and Dynamical Theorems."

AWARD OF THE MAKDOUGALL BRISBANE PRIZE.

2D BIENNIAL PERIOD, 1860–62. WILLIAM SELLER, M.D., F.R.C.P.E., for his Memoir of the Life and Writings of Dr ROBERT WHYTT, published in the Transactions.

AWARD OF THE NEILL PRIZE.

2D TRIENNIAL PERIOD, 1859–62. ROBERT KAYE GREVILLE, LL.D., for his Contributions to Scottish Natural History, more especially in the department of Cryptogamic Botany, including his recent papers on Diatomaceæ.

DIRECTIONS TO THE BINDER FOR PLACING THE PLATES IN THIS VOLUME.

Plate I. II.	Illustrating Dr John Denis Macdonald's Paper on the Order and Classification of Heteropoda, ... *To face page*	20
III.	Illustrating Mr Wm. Turner's Paper on the Structure of the *Chondracanthus Lophii*, with Observations on its Larval Form, . . .	76
IV.	Illustrating Mr Wm. Turner's Paper on the Structure of *Lerneopoda Dalmanni*, with Observations on its Larval Form, ...	88
V.	Illustrating Mr Edward Sang's Paper on the Deflection of the Plummet due to Solar and Lunar Attraction, ...	94
VI.	Illustrating Sir David Brewster's Paper on the Existence of Acari between the Laminæ of Mica in Optical Contact, ...	96
VII.	Illustrating Sir David Brewster's Paper on Certain Vegetable and Mineral Formations in Calareous Spar, ...	98
VIII.	Illustrating Professor William Thomson's Paper on the Secular Cooling of the Earth, ...	170
IX.	Illustrating Dr John Denis Macdonald's Paper on the Representative Relationships of the Fixed and Free Tunicata, regarded as two Sub-Classes of equivalent value (figs. 1, 2); on the Zoological Character of the living *Clio caudata*, as compared with those of *Clio borealis* given in Systematic Works (fig. 3); and Notes on the Anatomy of the Genus Firola (fig. 4), ...	192
X. XI.	Illustrating Sir David Brewster's Paper on the Structure and Optical Phenomena of Ancient Decomposed Glass, ...	204
XII.	Illustrating Sir David Brewster's Observations on the Polarization of the Atmosphere, made in St Andrews, ...	240
XIII.	Illustrating Professor Allman's Paper on a Pre-brachial Stage in the Development of Comatula, and its importance in relation to certain Aberrant Forms of Extinct Crinoids, ...	252
XIV. XV. XVI. XVII. XVIII.	Illustrating Dr R. E. Scoresby-Jackson's Paper on the Influence of Weather upon Disease and Mortality, ...	348
XIX.	Illustrating Sir David Brewster's Description of the Lithoscope, an Instrument for Distinguishing Precious Stones and other Bodies, . .	424
XX.	Illustrating Professor Kelland's Paper on Superposition, . . .	474
XXI. XXII.	Illustrating the Rev. Robert Boog Watson's Paper on the Great Drift Beds with Shells in the South of Arran, ...	546
XXIII. XXIV. XXV. XXVI. XXVII.	Illustrating Professor Smith's Paper on some points in the Metrology of the Great Pyramid, ...	706
XXVIII. XXIX.	Illustrating Dr J. B. Pettigrew's Paper on the Structure and Action of the Auriculo-Ventricular Valve, ...	806

ERRATA.

Page 21, line 4 from bottom, *for* value v *read* value of v.

... 23, line 7 from top, *for* $\cos t + \sin t$ *read* $\sin t - \cos t$.

... — line 4 from bottom, *for* e^{-t} *read* e^{-2t}.

... — line 3 from bottom, the first *minus* sign in the line should be *plus*.

... 26, line 2 from bottom, *for* 23° 23′ *read* 66° 37′.

LAWS

OF THE

ROYAL SOCIETY OF EDINBURGH.

AS REVISED 5TH JANUARY 1857.

LAWS.

[By the Charter of the Society (printed in the *Transactions*, Vol. VI. p. 5), the Laws cannot be altered, except at a Meeting held one month after that at which the Motion for alteration shall have been proposed.]

I.

THE ROYAL SOCIETY OF EDINBURGH shall consist of Ordinary and Honorary Fellows. Title.

II.

Every Ordinary Fellow, within three months after his election, shall pay Two Guineas as the fee of admission, and Three Guineas as his contribution for the Session in which he has been elected; and annually at the commencement of every Session, Three Guineas into the hands of the Treasurer. This annual contribution shall continue for ten years after his admission, and it shall be limited to Two Guineas for fifteen years thereafter.* The fees of Ordinary Fellows residing in Scotland.

III.

All Fellows who shall have paid Twenty-five years' annual contribution shall be exempt from farther payment. Payment to cease after 25 years.

IV.

The fees of admission of an Ordinary Non-Resident Fellow shall be £26, 5s., payable on his admission; and in case of any Non-Resident Fellow coming to reside at any time in Scotland, he shall, during each year of his residence, pay the usual annual contribution of £3, 3s., payable by each Resident Fellow; but after payment of such annual contribution for eight years, he shall be exempt from any farther payment. In the case of any Resident Fellow ceasing to reside in Scot- Fees of Non-Resident Ordinary Fellows. Case of Fellows becoming Non-Resident.

* At the Meeting of the Society, on the 5th January 1857, when the reduction of the Contributions from £3, 3s. to £2, 2s., from the 11th to the 25th year of membership, was adopted, it was resolved that the existing Members shall share in this reduction, so far as regards their future Annual Contributions.

A modification of this rule, in certain cases, was agreed to 3d January 1831.

land, and wishing to continue a Fellow of the Society, it shall be in the power of the Council to determine on what terms, in the circumstances of each case, the privilege of remaining a Fellow of the Society shall be continued to such Fellow while out of Scotland.

V.

Defaulters.

Members failing to pay their contribution for three successive years (due application having been made to them by the Treasurer) shall be reported to the Council, and, if they see fit, shall be declared from that period to be no longer Fellows, and the legal means for recovering such arrears shall be employed.

VI.

Privileges of Ordinary Fellows.

None but Ordinary Fellows shall bear any office in the Society, or vote in the choice of Fellows or Office-Bearers, or interfere in the patrimonial interests of the Society.

VII.

Numbers Unlimited.

The number of Ordinary Fellows shall be unlimited.

VIII.

Fellows entitled to Transactions.

The Ordinary Fellows, upon producing an order from the TREASURER, shall be entitled to receive from the Publisher, gratis, the Parts of the Society's Transactions which shall be published subsequent to their admission.

IX.

Mode of Recommending Ordinary Fellows.

No person shall be proposed as an Ordinary Fellow without a recommendation subscribed by *One* Ordinary Fellow, to the purport below.* This recommendation shall be delivered to the Secretary, and by him laid before the Council, and shall afterwards be printed in the circulars for three Ordinary Meetings of the Society, previous to the day of the election, and shall lie upon the table during that time.

X.

Honorary Fellows, British and Foreign.

Honorary Fellows shall not be subject to any contribution. This class shall

* "A. B., a gentleman well skilled in several branches of Science (*or Polite Literature, as the case may be*), being to my knowledge desirous of becoming a Fellow of the Royal Society of Edinburgh, I hereby recommend him as deserving of that honour, and as likely to prove a useful and valuable Member."

This recommendation to be accompanied by a request of admission signed by the Candidate.

consist of persons eminently distinguished for science or literature. Its number shall not exceed Fifty-six, of whom Twenty may be British subjects, and Thirty-six may be subjects of foreign states.

XI.

Royal Personages.

Personages of Royal blood may be elected Honorary Fellows, without regard to the limitation of numbers specified in Law X.

XII.

Recommendation of Honorary Fellows.

Mode of Election.

Honorary Fellows may be proposed by the Council, or by a recommendation (in the form given below*) subscribed by three Ordinary Fellows; and in case the Council shall decline to bring this recommendation before the Society, it shall be competent for the proposers to bring the same before a General Meeting. The election shall be by ballot, after the proposal has been communicated *viva voce* from the Chair at one meeting, and printed in the circular for the meeting at which the Ballot is to take place.

XIII.

Election of Ordinary Fellows.

The election of Ordinary Fellows shall take place at the Ordinary Meetings of the Society. The election shall be by ballot, and shall be determined by a majority of at least two-thirds of the votes, provided Twenty-four Fellows be present and vote.

XIV.

Ordinary Meetings.

The Ordinary Meetings shall be held on the first and third Mondays of every month from November to June inclusive. Regular Minutes shall be kept of the proceedings, and the Secretaries shall do the duty alternately, or according to such agreement as they may find it convenient to make.

XV.

The Transactions.

The Society shall from time to time publish its Transactions and Proceedings. For this purpose the Council shall select and arrange the papers which they shall

* We hereby recommend _____
for the distinction of being made an Honorary Fellow of this Society, declaring that each of us, from our own knowledge of his services to (*Literature or Science, as the case may be*), believe him to be worthy of that honour.

(To be signed by three Ordinary Fellows.)

_____

_____

_____

To the President and Council of the Royal Society of Edinburgh.

deem it expedient to publish in the *Transactions* of the Society, and shall superintend the printing of the same.

XVI.

How Published.

The Transactions shall be published in Parts or *Fasciculi* at the close of each Session, and the expense shall be defrayed by the Society.

The Council.

There shall be elected annually, for conducting the publications and regulating the private business of the Society, a Council, consisting of a President; Six Vice-Presidents, two at least of whom shall be resident; Twelve Councillors, a General Secretary, Two Secretaries to the Ordinary Meetings, a Treasurer, and a Curator of the Museum and Library.

XVII.

Retiring Councillors.

Four Councillors shall go out annually, to be taken according to the order in which they stand on the list of the Council.

XVIII.

Election of Office-Bearers.

An Extraordinary Meeting for the Election of Office-Bearers shall be held on the fourth Monday of November annually.

XIX.

Special Meetings; how called.

Special Meetings of the Society may be called by the Secretary, by direction of the Council; or on a requisition signed by six or more Ordinary Fellows. Notice of not less than two days must be given of such Meetings.

XX.

Treasurer's Duties.

The Treasurer shall receive and disburse the money belonging to the Society, granting the necessary receipts, and collecting the money when due.

He shall keep regular accounts of all the cash received and expended, which shall be made up and balanced annually; and at the last Ordinary Meeting in January he shall present the accounts for the preceding year, duly audited. At this Meeting, the Treasurer shall also lay before the Council a list of all arrears due above two years, and the Council shall thereupon give such directions as they may deem necessary for recovery thereof.

XXI.

Auditors.

At the Extraordinary Meeting in November, a Committee of three Fellows shall be chosen to audit the Treasurer's accounts, and give the necessary discharge of his intromissions.

The report of the examination and discharge shall be laid before the Society at the last Ordinary Meeting in January, and inserted in the records.

XXII.

General Secretary's Duties.

The General Secretary shall keep Minutes of the Extraordinary Meetings of the Society, and of the Meetings of the Council, in two distinct books. He shall, under the direction of the Council, conduct the correspondence of the Society, and superintend its publications. For these purposes, he shall, when necessary, employ a clerk, to be paid by the Society.

Secretaries to Ordinary Meetings.

The Secretaries to the Ordinary Meetings shall keep a regular Minute-book, in which a full account of the proceedings of these Meetings shall be entered; they shall specify all the Donations received, and furnish a list of them, and of the donors' names, to the Curator of the Library and Museum: they shall likewise furnish the Treasurer with notes of all admissions of Ordinary Fellows. They shall assist the General Secretary in superintending the publications, and in his absence shall take his duty.

XXIII.

Curator of Museum and Library.

The Curator of the Museum and Library shall have the custody and charge of all the Books, Manuscripts, objects of Natural History, Scientific productions, and other articles of a similar description belonging to the Society; he shall take an account of these when received, and keep a regular catalogue of the whole, which shall lie in the Hall, for the inspection of the Fellows.

XXIV.

Use of Museum and Library.

All articles of the above description shall be open to the inspection of the Fellows, at the Hall of the Society, at such times, and under such regulations, as the Council from time to time shall appoint.

XXV.

Register Book.

A Register shall be kept, in which the names of the Fellows shall be enrolled at their admission, with the date.

CONTENTS.

PART I. (1861-62.)

PAGE

I. *On the Anatomy and Classification of the Heteropoda.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." (With two Plates, I., II.), ... 1

II. *Investigation of an Expression for the Mean Temperature of a Stratum of Soil, in Terms of the Time of Year.* By JOSEPH D. EVERETT, M.A., Professor of Mathematics, &c., in King's College, Windsor, Nova Scotia, ... 21

III. *On a Difficulty in the Theory of Rain.* By JAMES DALMAHOY, Esq., 29

IV. *On the Pressure Cavities in Topaz, Beryl, and Diamond, and their bearing on Geological Theories.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., ... 39

V. *On the Theory of Numbers.* By H. F. TALBOT, Esq., . . 45

VI. *On the Rain-Fall in the Lake District in* 1861, *with some Observations on the Composition of Rain Water.* By JOHN DAVY, M.D., F.R.SS. Lond. & Edin., ... 53

VII. *On the Structure of the Chondracanthus Lophii, with Observations on its Larval Form.* By WM. TURNER, M.B. (Lond.), F.R.S.E., and H. S. WILSON, M.D., Demonstrators of Anatomy in the University of Edinburgh. (With a Plate, III.), . . 67

VIII. *On the Structure of Lerneopoda Dalmanni, with Observations on its Larval Form.* By WM. TURNER, M.B. (Lond.), F.R.S.E., and H. S. WILSON, M.D., Demonstrators of Anatomy. (With a Plate, IV.), ... 77

IX. *On the Deflection of the Plummet due to Solar and Lunar Attraction.* By EDWARD SANG, Esq. (With a Plate, V.), . . 89

PAGE

X. *On the Existence of Acari between the Laminæ of Mica in Optical Contact.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S. (With a Plate, VI.), ... 95

XI. *On Certain Vegetable and Mineral Formations in Calcareous Spar.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S. (With a Plate, VII.), ... 97

XII. *Memoir of the Life and Writings of Robert Whyft, M.D., Professor of Medicine in the University of Edinburgh from* 1747 *to* 1766. By WILLIAM SELLER, M.D., F.R.S.E., Fellow of the Royal College of Physicians of Edinburgh, ... 99

XIII. *Experimental Inquiry into the Laws of the Conduction of Heat in Bars, and into the Conducting Power of Wrought Iron.* By JAMES D. FORBES, LL.D., D.C.L,, F.R.S., V.P.R.S. Ed., Corresponding Member of the Institute of France, Principal of the United College of St Salvator and St Leonard, St Andrews, . 133

XIV. *On the Density of Steam.* By Professor W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS. Lond. and Ed., &c., . . 147

XV. *On the Secular Cooling of the Earth.* By Professor WILLIAM THOMSON, LL.D., F.R.S., F.R.S.E. (With a Plate, VIII.) . 157

PART II. (1862-63.)

XVI. *On the Representative Relationships of the Fixed and Free Tunicata, regarded as two Sub-Classes of equivalent value; with some General Remarks on their Morphology.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (With a Plate, IX.), . 171

XVII. *On the Zoological Characters of the Living* Clio caudata, *as compared with those of* Clio borealis *given in Systematic Works.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (With a Plate, IX. fig. 3), ... 185

PAGE

XVIII. *Notes on the Anatomy of the Genus Firola.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (With a Plate, IX. fig. 4), 189

XIX. *On the Structure and Optical Phenomena of Ancient Decomposed Glass.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., &c. (With two Plates, X., XI.), ... 193

XX. *On the Polarisation of Light by Rough and White Surfaces.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., &c., . . 205

XXI. *Observations on the Polarisation of the Atmosphere, made at St Andrews in* 1841, 1842, 1843, 1844, *and* 1845. By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., &c. (With a Plate, XII.), 211

XXII. *On a Pre-Brachial Stage in the Development of Comatula, and its importance in relation to certain Aberrant Forms of Extinct Crinoids.* By Professor ALLMAN. (With a Plate, XIII.), . 241

XXIII. *Some Account of the Recent Progress of Sanskrit Studies.* By J. MUIR, D.C.L., LL.D., ... 253

XXIV. *On Fagnani's Theorem.* By H. F. TALBOT, Esq., . . 285

XXV. *On the Influence of Weather upon Disease and Mortality.* By R. E. SCORESBY-JACKSON, M.D., F.R.S.E., F.R.C.P., Lecturer on Materia Medica and Therapeutics at Surgeons' Hall, Edinburgh. (With Five Plates, XIV.–XVIII.), . . . 299

XXVI. *On the Anatomical Type of Structure of the Human Umbilical Cord and Placenta.* By J. Y. SIMPSON, M.D., Professor of Medicine and Midwifery in the University of Edinburgh, . . 349

XXVII. *On Earth-Currents during Magnetic Calms, and their connection with Magnetic Changes.* By BALFOUR STEWART, M.A., F.R.S., . 355

XXVIII. *On the Great Refracting Telescope at Elchies, in Morayshire, and its Powers in Sidereal Observation.* By Professor C. PIAZZI SMYTH, ... 371

PART III. (1863–64.)

PAGE

XXIX. *Description of the Lithoscope, an Instrument for distinguishing Precious Stones and other bodies.* By Sir DAVID BREWSTER, K.H., F.R.S. (With a Plate, XIX.), . . . 419

XXX. *On the Agrarian Laws of Lycurgus, and one of Mr Grote's Canons of Historical Criticism.* By Professor BLACKIE, . 425

XXXI. *On the Limits of our Knowledge respecting the Theory of Parallels.* By Professor KELLAND, ... 433

XXXII. *On the Temperature of Certain Hot-Springs in the Pyrenees.* By R. E. SCORESBY-JACKSON, M.D., F.R.C.P.E., Lecturer on Materia Medica and Therapeutics at Surgeons' Hall, Edinburgh, 451

XXXIII. *On Superposition.* By the Rev. PHILIP KELLAND, M.A., F.R.S., Professor of Mathematics in the University of Edinburgh. Part II. (Continued from Vol. XXI. p. 273.) (With a Plate, XX.), ... 471

XXXIV. *On the Variations of the Fertility and Fecundity of Women according to Age.* By J. MATTHEWS DUNCAN, M.D., . . 475

XXXV. *On the most Volatile Constituents of American Petroleum.* By EDMUND RONALDS, Ph.D., ... 491

XXXVI. *On Sun-Spots and their Connection with Planetary Configurations.* By BALFOUR STEWART, M.A., F.R.S., . . . 499

XXXVII. *On the freezing of the Egg of the Common Fowl.* By JOHN DAVY, M.D., F.R.SS. Lond. and Ed., &c. (Communicated by Professor MACLAGAN), ... 505

XXXVIII. *On the Morphological Relationships of the Molluscoida and Cœlenterata, and of their leading Members,* inter se. By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus," . 515

XXXIX. *On the Great Drift Beds with Shells in the South of Arran.* By the Rev. ROBERT BOOG WATSON, B.A., F.R.S.E., Hon. Mem. Nat. Ver. Lüneburg. (With two Plates, XXI., XXII.), . 523

PAGE

XL. *On the Principal Deities of the Rigveda.* By J. Muir, D.C.L., LL.D., ... 547

XLI. *The Law of the Volumes of Aeriforms extended to Dense Bodies.* By Rev. J. G. Macvicar, M.A., D.D., Moffat, . . . 581

XLII. *Biographical Sketch of Adam Ferguson, LL.D., F.R.S.E., Professor of Moral Philosophy in the University of Edinburgh.* By John Small, M.A., Librarian to the University, . . 599

XLIII. *On the Reputed Metrological System of the Great Pyramid.* By Professor C. Piazzi Smyth. (With five Plates, XXIII.–XXVII.), 667

XLIV. *On the Theory of Isomeric Compounds.* By Dr A. Crum Brown, 707

XLV. *On the Theory of Commensurables.* By Edward Sang, Esq. . 721

XLVI. *On the Structure and Action of the Auriculo-Ventricular Valves.* By James B. Pettigrew, M.D. Communicated by W. Turner, M.B., Demonstrator of Anatomy in the University of Edinburgh, ... 761

Proceedings of Statutory General Meetings, &c., ... 807
List of Members Elected, ... 814
List of the present Ordinary Members, in the order of their Election, . . 816
List of Non-Resident and Foreign Members, elected under the Old Laws, . 823
„ *Honorary Fellows*, ... 823
„ *Fellows Deceased, Resigned, and Cancelled, from* 1861 *to* 1864, . 825
Public Institutions, &c., entitled to receive the Transactions and Proceedings of the Society, ... 826
List of Donations continued from Vol. XXII., p. 750, . . . 828
Index, ... 857

TRANSACTIONS.

I.—*On the Anatomy and Classification of the Heteropoda.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." (Plates I., II.)

(Read 30th January 1862.)

Notwithstanding the rapid progress of zoology in other departments, the *Heteropoda* still remain imperfectly known, if one may form a judgment from the scantiness of definite information respecting them to be found in systematic works. Nearly all the available space is usually occupied with an exposition of the errors and doubts of the great men who gave us the first outlines of the order, while comparatively little is done to improve the subject, or make it intelligible to the student.

Having had favourable opportunities of examining all the veritable genera of existing *Heteropoda*, I have attempted the arrangement of my notes in a connected form; and as they have been taken directly from nature, I am led to hope that some little may be thus added to what may be already known of the particular species investigated.*

It is usual to divide the *Heteropoda* into two families,—viz., *Firolidæ* and *Atlantidæ;* but it appears to me that there is as little difference between *Oxygyrus* and *Cardiapoda*, as there is between the latter genus and *Firoloides*, while all three differ sufficiently *inter se* to warrant their separation into three distinct families, in each of which two well-defined genera may be included; thus—

1. Firolidæ,	*Firoloides* (Lesueur),	*Firola* (Perou and Les.)
2. Carinariidæ,	*Cardiapoda* (D'Orb.),	*Carinaria* (Lamarck).
3. Atlantidæ	*Oxygyrus* (Benson),	*Atlanta* (Lesueur).

* Not having had the opportunity of consulting many of the original figures of the French naturalists, I have to acknowledge the great advantage I have derived from the study of Mrs GRAY'S excellent etchings, in helping me to determine species, as also the descriptive letterpress of Dr GRAY. Many of the anatomical particulars detailed by me had, of course, been previously observed by others; and though I have not clogged the paper with the separate announcements, dates, and authorities of every addition to our knowledge of *Heteropoda*,—which, indeed, would be no small task,—I can vouch for it, that all the facts embodied in the text have fallen under my own observation, and they may therefore be regarded either in one sense as original matter, or in another, as confirmation of what had been already made known.

BRITISH MUSEUM

The zoological characters of these three families and their genera are given in the following Table of Classification :—

Heteropoda.

I. Gymnosomata* (Firolidæ). Animal wholly naked or without a shell.
   1. With slender tentacula, and destitute of true branchiæ. Visceral mass near the root of the filiform process of the metapodium,—*Firoloides*.
   2. With rudimentary or no tentacula, but furnished with true branchiæ. Visceral mass considerably in advance of the base of the filiform process of the metapodium,—*Firola*.

II. Thecosomata inoperculata (Carinariidæ). Animal in great part naked, but having the visceral mass protected by a shell.
   1. Shell corneous, with an involute nucleus. Swimming-plate nearly opposite the visceral mass. Metapodium with filiform appendage,—*Cardiapoda*.
   2. Shell calcareous, with spiral nucleus. Swimming-plate considerably in advance of the visceral mass. Metapodium laterally compressed, without filiform appendage,—*Carinaria*.

III. Thecosomata operculata (Atlantidæ).
   1. Shell corneous, with an involute nucleus. Operculum subtrigonal, with small lateral subapical nucleus,—*Oxygyrus*.
   2. Shell calcareous, with a spiral nucleus. Operculum oval, with a large median subapical nucleus,—*Atlanta*.

I have thus far anticipated myself in the construction of the preceding table, but as it affords a bird's-eye view of the subject, it will be found convenient for reference as occasion may require.

General Outline of the Order.

The *Heteropoda* are pre-eminently distinguished by the laterally compressed and fin-like configuration of the body of the foot and propodium; the rudimentary state of the creeping disc (*mesopodium*), and the great length of the operculigerous lobe (*metapodium*), or its homologue, often continued into a kind of caudal appendage.

The remarkable transparency of the tissues of these animals reveals a great part of their internal structure to the anatomist without dissection, which is often a matter of great difficulty, arising from the same circumstance. They are all furnished with a cylindrical proboscis-like muzzle, and a well marked neck, large, mobile, and singularly beautiful eyes, lying in socket-like spaces, though invested with the common integument at the posterior part of the base of simple conical tentacula, except in the true *Firolæ*, in which the tentacula are absent, or at most very rudimentary.

The auditory sacs in all contain single spherical otoliths, increasing by exogenous layers, and revolving like planets on their axes, by the action of vibratile cilia lining the sacs. The large size of the lenses of the eyes, as compared with that of the otoliths, affords a character, which, although of a relative nature, is

* I have made use of the convenient terms *Gymnosomata* and *Thecosomata* in a similar sense to that in which De Blainville applied them to the *Pteropoda*.

of much importance in distinguishing the *Heteropoda*. In *Cardiapoda* and *Carinaria*, as well as in the naked genera, the acoustic sacs are, as it were, appended to the auditory nerves, which are of considerable length, and arise from the supra-œsophageal ganglia. In the latter particular the heteropods differ from most of the true gasteropods, in which the special centre of audition is incorporated with the subœsophageal ganglia.

The oral aperture is circular, and at the extremity of a lengthy muzzle, within which the "buccal," or rather the lingual mass, appears to occupy but a small space. There are no labial or maxillary dental organs, but the lingual armature is highly characteristic of the order. In accordance with the laws of its development, the lingual ribbon gradually increases in breadth from before backwards. The rachis consists of a single series of plates, while there are three members in each pleura; and all the dental points are simple, sharp, and conical, either perfectly straight and projecting backwards, or slightly curved and inclined more or less inwards.

The segments of the rachis bear a variable number of simple teeth; but the internal pleural plates, whose attached surface occupies nearly, or quite the whole breadth of the pleuræ, generally present one large dental process at the inner extremity, with a much smaller one somewhere near its base. The absence of this smaller tooth, or, when present, its internal or external position with respect to the larger one, may be taken into account in classification; but as the two outer members of the pleuræ in all the *Heteropoda* are in the form of simple claw-shaped uncini, any specific characters afforded by them can only be of a relative nature, as to proportionate length, amount of curvature, thickness, &c. On exhibiting my preparations and drawings to my respected friend, Mr W. S. Macleay, he saw very plainly that generic characters might be drawn from the rachis alone; and it was with no small satisfaction that I enjoyed the concurrence of so great a man in my first attempt to effect a classification of the *Heteropoda*, by their lingual dentition, which so often outlives the decay of the soft parts generally.

The gills, or branchiæ, usually consist of a linear series of short claviform or tapering processes, with a loose cellular investment, presenting a longitudinal zigzag fold on the inner surface, giving rise to the plumose appearance sometimes so incorrectly represented in figures of these animals. In *Firoloides*, however, I have never seen any vascular appendages of the nature of gills, and they are frequently absent, or inconspicuous even in *Atlanta*.

In closing this general sketch, it only remains to be stated, that the sexes are distinct in *Heteropoda*, as was first suspected by M. Laurillard, who assisted the great Cuvier in his dissections. Mr Macleay further informed me that Cuvier, on the same account, was deterred from placing them amongst his *Tectibranchiata*. Modern anatomists, however, have thought fit to contradict this opinion, though upon what grounds, except hearsay, it is difficult to determine.

I had originally intended to pass all the genera in review, dealing with the whole anatomy of each in the order observed in the foregoing table; but this would extend the paper beyond reasonable limits, and perhaps prove monotonous to the reader, from the frequent repetitions of essentially the same anatomical particulars, however striking their modifications may be, in the different members of this natural order.

I shall therefore first make selection of *Firoloides* and *Atlanta* for especial description, occupying, as I conceive them to do, the two extremes of the group; and those modifications to which I have alluded may be briefly noticed in a passing glance at the remaining genera, or at least the particular species which have casually fallen under my observation.

Firoloides (Lesueur).

While cruising in the South Seas, and subsequently in the neighbourhood of the West India Islands, I was fortunate enough to obtain numerous specimens of a solitary species of *Firoloides*, and which I find, if I am not very much mistaken, has been several times named by different writers who may have met with it under such deceptive conditions as to mask its identity. Thus, the male and female of this species are represented in Plate XVI., *Voy. la Bonite*, figs. 8 and 1 respectively, though the former has been designated *Firola de Keraudren*, as also *Eydouxii*, and the latter *F. de Desmarest*. I have traced it, moreover, under other names in certain less definite figures, illustrating the works of the French naturalists, who appear to have had most to do with the *Heteropoda* from the very foundation of the order, though it is scarcely reconcilable with Lesueur's figure of *Firola Demerastiana* (l, c, i, 39, t. 2, f, 1), which Dr GRAY seems to regard as the type of the genus *Firoloides*. Now, though all this may be justly regarded as a stumbling-block to the student, he has still more to encounter in the literature of other genera. I shall therefore eschew the naming mania altogether, and simply attempt succintness of description, aided by fidelity in the figures, so as to render the species in question just as definite as if I were to add fresh synonyms to the existing confusion. It is a cheering reflection, however, to know that we have in the lingual dentition a guide that cannot be gainsayed, and whose expunging power may be safely applied to a host of *mis*-applied cognomina with their bracketed italics.

On entering Bass' Strait in H.M.S. "Torch," I obtained my first *Firoloides*, which was so perfectly transparent that, but for the brilliancy of its eyes, relieved by the dark pigment coat protecting the retinæ, it would probably have passed unobserved. When immersed in sea-water, the eyes were seen rapidly swaying from side to side with the undulatory movement of the elongated and pliant body, no other parts being visible but the faintly rose-tinted visceral nucleus and the little buccal mass.

The whole length of the animal did not exceed an inch and a quarter; and when placed under the microscope, the principal features of its anatomy were traceable through the integuments. (Plate I. figs. 1, 2, 3 and 4.)

The head supported two delicate tentaculæ of moderate length and taper form, and a large and beautiful eye occupied a considerable dilatation at the back part of the base of each. The muzzle was rather slender, with a small truncated extremity, within which was contained a minute buccal mass, lodging a short lingual sac. At the posterior part of this organ, the buccal artery terminated between two nodules of nervous matter (*buccal ganglia*) (Plate I. fig. 2, d), while a narrow œsophagus proceeded from its upper and fore parts.

The principal viscera were clustered together at the hinder extremity of the body, which is abruptly truncated above, but produced inferiorly into a suddenly tapering tail (Plate I. fig. 4, g), terminating in a filamentous appendage (Plate I. fig. 1, n, fig. 4, h), marked at certain intervals by slightly dilated joints or rings, softly tinted with brownish pigment. This appendage is highly sensitive and mobile, trailing in an undulatory manner after the animal as it swims. I can safely say that it is neither a tapeworm nor an oviduct, though I am as yet unacquainted with its use.

The characteristic laterally compressed and fan-like foot (Plate I. fig. 1, g) of the *Heteropod* crested the ventral surface of the body at a little distance posterior to the centre, and near the fore part of the free margin of this organ a minute sucker-disc (Plate I. fig. 1, f) was distinctly visible.

The body in this species is everywhere enveloped by a perfectly transparent homogeneous-looking integument, the inner surface of which is studded at irregular intervals with small clusters of cells, surrounding a larger vesicle (most probably cutaneous glands). Internal to this there is a stout muscular sheath (Plate I. fig. 2, o), which includes the buccal mass in front, and the visceral nucleus behind where it extends into the tail, the homologue of the operculigerous lobe of the foot in ordinary gasteropods.

The preceding sketch will suffice to give a general idea of the animal; but we shall now proceed to consider more in detail the anatomy of the organs of sense and the several organic systems, which will enable us to understand more thoroughly the modifications of this type occurring in the other genera.

Organs of Sense and Nervous System.—The eyes of *Firoloides* (Plate I. fig. 1, b. fig. 2, l), like those of other *Heteropoda*, are very large and beautiful, and situate at the posterior part of the base of the slender tentacula. They are invested by the common integument, and exhibit a very remarkable structure. A little bulb, somewhat compressed from above downwards and constricted in front, forms the body of each. This is usually of a pale reddish-brown tint, and sparingly lined with darker pigment granules. A perfectly spherical lens of highly refracting material rests in a depression at its fore-part, and the lens itself is capped over by a strongly

curved meniscus, in which a cell-structure is often distinctly visible beneath the cutaneous covering.

The acoustic capsules (Plate I. fig. 1, c, fig. 2, m) contain each a transparent globular otolith, which is very much smaller than the lens of the eye. Several little prominences, on the inner surface of the capsules, represent those which poise off the crucial otolith of *Sepia*, from the wall of the vestibule in which it lies. The ear-sacs, moreover, appear to float freely within the muscular sheath, being connected with the cerebroid ganglia by long and delicate auditory nerves.

The nervous system of this creature, from the peculiar mode of distribution of its ganglia and communicating or commissural cords, not a little resembles the *homo*-gangliate type.

Two elliptical knots of nervous matter lying side by side, and blended together by their contiguous borders, compose the cerebroid ganglia (Plate I. fig. 2, i) in which the combination of several smaller centres may be distinctly traced in recent specimens.

Two large nerve-trunks, derived from the cerebroid ganglia anteriorly, communicate with the before-mentioned buccal ganglia, lying at the posterior extremity of the lingual cartilages, and give off nerves to the buccal mass and mouth.

The optic nerves (Plate I. fig. 2, h) emerge from the posterior part of the outer border of the cerebroid ganglia, by a club-shaped base, and taper gracefully towards the eyes, at the back part of which they form a remarkably thick retinal expansion.

The delicate auditory nerves arise immediately behind the optic, and thence passing outwards and backwards some little distance, reach the acoustic sacs (Plate I. fig. 1, c, fig. 2, m), upon the walls of which other more delicate filaments are also distributed. On examining the origin of the larger nerves, it would appear as though they communicated with each other in the median line by very faint striæ traversing the substance of the cerebroid ganglia. From the posterior border of the latter bodies, two large nervous chords (Plate I. fig. 2, n) pass directly backwards, including between them the œsophagus (Plate I. fig. 1, e, fig. 2, g) and buccal artery (Plate I. fig. 2, e), and having reached the root of the swimming-fin, they join the fore part of the pedal ganglia (Plate I. fig. 1, h, fig. 3, a)—two nearly circular but laterally compressed masses of nervous matter connected with one another by their contiguous surfaces.

These ganglia give off numerous branches to the neighbouring parts, but from the inferior border of each a special nerve descends with the pedal artery, and is ultimately distributed to the swimming-plate, while two stout trunks, arising from the posterior part of the ganglia, blend together in a loose plexiform manner, and course backwards as a single chord. This at first accompanies the œsophagus, to which it supplies several branches, next sends off a nerve of communication to the visceral ganglia, and, finally subdividing in the tail, is lost in the filamentous appendage.

As far as I have yet discovered with any certainty, the visceral ganglia are two in number—viz., 1st, A minute nodule of neurine (Plate I. fig. 4, d), joined by the commissural nerve above mentioned, and situate at the angle between the origin of the visceral artery and the continuation of the main trunk; and, 2dly, a much larger oblong subquadrilateral ganglion (Plate I. fig. 4, e), lying on the right wall of the intestine near its origin. From the superior angles of the latter body distinct branches take their rise; one in particular joining the former ganglion, while others are distributed to the heart and everted mantle with its sphincter-like opening. The digestive and internal generative organs are supplied with nerves from the inferior angles of the same nervous centre.

Digestive System.—The proboscis of *Firoloides* is susceptible of retraction and protrusion to a considerable extent, in which movements the fore-part of the common muscular sheath, and even the œsophagus itself, plays an important part (Plate I. fig. 2, g).

The oral orifice (Plate I. fig. 2, a) is terminal, rounded, and unfurnished with labial plates or mandibles of any kind. The buccal mass (Plate I. fig. 2, d) is small, and placed near the extremity of the muzzle, as above-mentioned. The lingual cartilages, in which the cell structure is beautifully marked, are oval in shape, wrapped together by ligaments, and supplied with muscles to effect their varied movements.

The tongue-sac is scarcely longer than the cartilages, terminating posteriorly in a rounded extremity, and the dental area exhibits a remarkable increase in its breadth from before backwards. The rachidian plates (Plate I. fig. 5, o) being concave both in front and behind, are broadly (H) shaped, and bear a large central tooth, with a little comb of denticles on either side. The first or inner pleural plates (Plate I. fig. 5, 1) are destitute of a small inner tooth near the base of the large cusp, and the uncini very nearly equal the breadth of the pleuræ, as given in Plate I. fig. 5.

An elongated and somewhat flattened salivary gland (Plate I. fig. 2, c) lies along the lingual cartilage on either side, and communicates with the mouth by a very short duct.

The œsophagus, proceeding from the upper and fore-part of the buccal mass, takes a course directly backwards, in close relationship with the buccal artery and nerves, and having reached the visceral nucleus, the canal exhibits a slight gastric enlargement, which receives the biliary ducts inferiorly from the supero-posterior wall of the stomach; a short intestine passes upwards and backwards between the heart and the abdominal viscera, and terminates in the anus (Plate I. fig. 4, o) on a little prominence above the latter organs, and below and between two small anal lobes or leaflets (Plate I. fig. 4, p).

The lining membrane of the stomach and intestine is richly ciliated, but the cilia increase both in size and activity as they approach the vent, around which they may be very distinctly observed. The apparently undulatory movement

produced by the consecutive action of the cilia within the canal, proceeds in a forward direction, though of course the current to which it gives rise takes an opposite course.

As compared with the bulk of the animal, the liver (Plate I. fig. 4, f) is of small size; and its minute lobuli may be traced along the posterior wall of the short intestine and stomach, and for some little distance farther, in front of the internal organs of generation, which occupy the remainder of the visceral chamber.

The Mantle, Respiratory and Circulatory Systems.—In *Atlanta*, *Carinaria*, and *Cardiapoda* the mantle permanently envelopes the viscera, and, keeping pace with the development of these organs, furnishes the materials of growth to the protecting shell. The course of the intestine, and the position of the heart and respiratory surface, all of which constantly preserve a definite relationship to each other, exhibit, with reference to the extremities of the body, the usual arrangement prevailing amongst the so-called *præso-branchiate* gasteropoda,—that is, the alimentary canal being bent upon itself, the rectum passes forwards, and the anus terminates it anteriorly; the branchiæ also lie in advance of the heart, through which the circulation proceeds in a backward direction. In *Firoloides*, on the contrary, the mantle is everted and thrown backwards; and the parts above mentioned being more or less intimately connected with it, suffer a remarkable change of position, which will be better understood when we have considered the anatomy of the organs contained in the visceral nucleus. Thus, the free border of the mantle is, as it were, turned inside out, and so very much constricted as to circumscribe a small oval opening (Plate I. fig. 4, n) on the right side of the visceral mass, and immediately in front of the rectum. This opening is surrounded by a little sphincter muscle, which is intersected by numerous radiating fibres, so that ample provision is made for its contraction and dilatation, frequently observed with a degree of rhythmical precision in very recent specimens.

I have never seen any branchial appendages, properly so called, in *Firoloides*, although one would imagine, had such been present, that they would be still more apparent in consequence of the eversion of the mantle. Apart from the idea that the general surface of the body may be more or less subservient to respiration, the position of the auricle of the heart points out the locality in which we might expect to find the organs especially adapted to this function. It must be observed, however, that immediately behind the heart, and above the anal lobes, a richly ciliated space or fossa (Plate I. fig. 4, q), with a prominent margin, is constantly present; and I am inclined to think that the little clusters of spherical cells, represented in Plate I. fig. 4, r, as occurring upon the pallial wall of the pericardium, may possibly be concerned in respiration, and in some way associated with the modification of the mantle above explained.

The heart, consisting of a single auricle and ventricle, rests upon the mantle-chamber which lies between it and the rectum. The muscular fibres of the auricle

(Plate I. fig. 4, s) divide and interlace with one another, leaving angular and irregular spaces between them. They are not so clearly to be traced in the ventricle (Plate I. fig. 4, t), the walls of which, nevertheless, seem to be somewhat thicker than those of the auricle. A circular constriction marks the union of the two chambers, and two valves guard the auriculo-ventricular opening. The base of the aorta is slightly dilated and highly contractile, being furnished with valves at its origin from the ventricle. It very soon gives rise to a large vessel which passes downwards and backwards to supply the viscera; but the great podocephalic division (Plate I. fig. 4, b) crosses over the alimentary tube from the left to the right side, and, having formed a few flexures, runs forwards alongside the œsophagus until it arrives a little in front of the pedal ganglia, where it curves downwards to give rise to the pedal artery (Plate I. fig. 3, c), which opens directly into the great ventral sinus at the root of the swimming-plate. From the origin of the latter vessel the main trunk courses forwards as the buccal artery, and, having reached the fundus of the tongue-sac, it is no further traceable.

Generative System.—The external male organ lies on the right side of the body near its posterior extremity, and consists of two portions, one of which is of considerable length, terminating in a bulbous enlargement (Plate I. fig. 1, m), but the other is short, with the orifice or terminal part usually inverted. The follicles of the testicle converge to a duct, which exhibits a fusiform, though twisted dilatation (Plate I. fig. 1, l) in its course, and which is, moreover, distinguished by its coating of black pigment cells; but the duct does not appear to reach the external organ, or, if it does in this case, nothing of the kind is discoverable in the other genera of Heteropoda.

The female orifice (Plate I. fig. 4, i) is situated immediately above the root of the tail, and guarded on each side by a small laterally compressed leaf-like process, somewhat larger than the anal lobes previously noticed. The oviduct (Plate I. fig. 4, l) is rather capacious, and once or twice doubled upon itself. The ovarian sacculi communicate with its inner extremity; and the impregnated ova, with which the duct is sometimes found distended, may be observed in all stages of yelk-cleavage, exhibiting further advancement as they approach the external opening, from which a delicate nidamental chord with ova, in single or two alternating series, may be frequently seen protruding (Plate I. fig. 6).

Atlanta (Lesueur).

As the remarks which I have to make on the anatomy of *Atlanta* have wide reference to all the species of the genus, I shall defer the perplexing question of specific determination to a future paper.

General Sketch of the Genus (Plate II. Fig. 1).

The shell of *Atlanta* is dextrally-spiral in the young state; but it subse-

quently becomes plan-orbicular, laterally compressed, and nearly symmetrical, bearing a dorsal keel of variable depth, literally nothing more than a thin fold of the shell itself, which thus presents a deep notch or slit at the corresponding part of the outer lip.

The animal admits of complete retraction within its shell, and is furthermore protected by a delicate oval operculum, with a large *sinistrally*-spiral median, and subapical nucleus (Plate II. fig. 1′).

The head of *Atlanta* is supported by a kind of neck, with which the proboscis forms an angle more or less obtuse. The eyes are proportionately very large, and fronted by small conical tentacula; and all the parts of the foot are more compact than in *Firoloides*. Thus, the part corresponding with the tail in the latter genus is an obvious metapodium, bearing an operculum in the former. The swimming-plate is relatively stronger, and the sucker-disc better defined. The visceral mass also bears a larger proportion to the rest of the body.

More Particular Description of the Foot and Retractor Muscle.

The muscular fibres which fix *Atlanta* to the shell arise from a short oblique line commencing near the nucleus, and extending some little distance outwards and forwards on the upper or right wall of the tube. From this origin they pass round the columellar wall, gradually diverging from one another as they approach the mouth of the shell. Here they commence to blend with the proper muscular fibres of the foot, but more especially with those of the operculigerous lobe. This lobe forms the posterior part of the foot, from the inferior surface of the base of which the vertical fin bearing the sucker-disc springs. Thus the foot altogether may be said to consist of three distinct portions—viz., the swimming-fin (Plate II. fig. 1, j) in front, the operculigerous lobe (Plate II. fig. 1, θ) behind, and the sucker-disc (Plate II. fig. 1, k), holding an intermediate position. These structures are mainly composed of a basis of muscular and areolar tissue, overlaid with common integument. The muscular fibres are disposed in ribbed bundles at the central parts requiring the greatest support, and cross one another diagonally over the general surface, so that mobility in every direction is amply provided for; but as all the parts of the foot lie in one vertical plane, corresponding with that of the shell, all the lateral movements of the animal are necessarily the more vigorous.

The fin-shaped swimming-plate supports the mesopodium or sucker-disc on its posterior border near the base. The latter disc is divided by a longitudinal depression into two lateral lips or lobes, which meet together when the organ is contracted; but when it is fully expanded and brought into action, after the manner of its homologue, the creeping disc of the true gasteropod, it exhibits a more circular form, and as I have particularly observed in the case of *Oxygyrus*, glides over any resisting surface with imperceptible undulations. Into its central or concave part a muscular fasciculus, derived from the fibres of the great retrac-

tor, is fixed, so as, probably, to assist in forming a vacuum when the animal is entirely supported by this small disc; and the epithelial cells on its outer surface are frequently tinted with a yellowish, red, or purple pigment.

The metapodium bears the operculum (Plate II. fig. 1, λ, and fig. 1′.) on its dorsal surface, and terminates in a point posteriorly. The inferior surface of this curiously constructed tail-piece presents a median vertical fold or reel, which gradually decreases in depth as it approaches the pointed extremity where it ends. On either side of this fold is a little fossa, deepening in front, and passing some little distance within the anterior margin. To this latter part is appended a small moveable and valve-like process (Plate II. fig. 1. *i*), whose use I have not yet discovered; but it is probable that it has some office in directing the path of the respiratory currents, when the animal is retracted into its shell; and this view is favoured by the fact that the floor of each fossa is richly ciliated, with the undulations proceeding from behind forwards.

Organs of Sense and Nervous System.—The eyes (Plate II. fig. 1, e), as above-mentioned, are proportionately very large, and situated immediately above and in front of the cerebroid ganglia, capped over by a portion of common integument, which bulges out to enclose them. The outer and fore-part of this covering is beautifully rounded, smooth, and transparent, so as to form a kind of external cornea, lying directly over a brilliant spherical lens. Commencing at a zone corresponding with the limits of the cornea, the cell-pavement of the integument, and the deeper muscular tissue, become more apparent; and in front of each cornea, towards its inner side, is a conical and contractile tentaculum (Plate II. fig. 1, c) of small size compared with that of the eye. The lens, which is compressible to a certain extent, and invested with a capsule of homogeneous membrane, lies in front of a cylindrical case, which is somewhat fuller at the inner and posterior part, where the optic nerve expands into the retina, and all stray luminous rays are absorbed by a dense coating of reddish-purple or black pigment cells extending forwards to the lens, upon which it encroaches in an annular form to about one-fourth of its diameter.

At the inner side of the base of each eye there is a small projection upon which it rolls, obedient to the action of numerous small muscular bands that spring from the borders of a kind of socket, and are inserted into the organ at different points of its circumference. Thus the eyes enjoy a considerable range of motion, quite independent of the outer covering; and often, when the bright lens rolls within the margin of the pigmentary coating, the singular appearance of winking is produced. Both eyes generally move in concert, but each organ enjoys its own intrinsic movements irrespective of the other.

The acoustic capsules (Plate II. fig. 1, g), consisting of homogeneous membrane with a ciliated lining, contain each a single vitreous-looking otolith, usually with a small cavity in its centre, around which the successive deposit of concentric layers

is distinctly apparent. In *Firoloides*, *Firola*, *Cardiapoda*, and *Carinaria*, the auditory nerves are of considerable length, so that the ear-sacs are, as it were, appended to them; but in *Atlanta*, which embraces more of the characters of the true gasteropods, those sacs are closely applied to their proper ganglia.

The gullet and anterior or buccal division of the aorta pass through a nervous collar consisting of four supra (Plate II. fig. 1. f), and two sub-œsophageal (Plate II. fig. 1, i) (pedal) ganglia, connected by two lateral commissural chords, each of which arises from the two superior ganglia of the corresponding side by distinct slips. The two anterior cerebroids supply the short but stout optic nerves to the eyes, while the acoustic sacs lie in the outer angle between these and the posterior ganglia, having no connection whatever with the sub-œsophageal or pedal nervous bodies.

The two principal nerves supplied to the buccal mass and mouth arise from the anterior superior ganglia, pass forward on either side of the buccal artery, and at some little distance behind the lingual cartilages divide into a superior and inferior branch, and thus, communicating also with the buccal ganglia, are equally distributed to the muscular and other structures in this locality.

The pedal ganglia give off branches to the several parts of the foot, and to the muscular sheath of the body, and two stout commissural nerve-trunks arise from their posterior border and join the visceral ganglia, which preside over the heart's action and the functions of the neighbouring organs and parts. As there is much difficulty in tracing out the filaments derived from those ganglia in *Atlanta*, their homologues in *Firoloides* may be studied with advantage.

Digestive System.—Although the cylindroid proboscis exhibits great pliancy in itself, the range of its movements is much augmented by the mobility of the neck. Destitute of labial teeth, the lining membrane of the mouth is prolonged into the fauces and œsophagus above and behind, being continuous with the lining of the tongue-sac inferiorly, blending with the borders of the dental ribbon, which doubles over the fore-part of the supporting cartilages (Plate II. fig. 1, h), and thus projects into the oral cavity. The cartilages just noticed form the basis of the tongue, and consist of two principal pieces of an oblong figure, forming, by their union along the mesial line, a grooved pully-like surface, over which the lingual strap glides. In many gasteropods, a smaller piece of cartilage is articulated to the posterior extremity of each of the principal ones, so as to admit of greater mobility, and a certain amount of compression in the longitudinal direction; and I have reason to believe that corresponding pieces exist in the framework of the tongue of *Atlanta*. The cartilages are connected together and acted upon by transverse and oblique slips of muscular fibres, and small bundles pass from the inner surface of the tegumentary covering, to be inserted into them at different points.

The lingual strap, commencing by a small point beneath the anterior extremity of the basal cartilages, at first passes a little forwards, then turns upwards and

backwards, gradually increasing in breadth, and finally forms the floor of the tubular tongue-sac.

The rachidian plates (Plate II. fig. 5, g, fig. 8, o) are quadrilateral, and thicker posteriorly than in front, so that the corresponding angles are much more clearly defined. Each plate bears a single sharp conical tooth, arising directly from the middle of the posterior border.

The anterior and internal part of the first series of pleural plates (Plate II. fig. 8, 1.) abuts upon the members of the rachis, and there is never a small tooth on the inner side of the large terminal fang, though there is very generally a characteristic minute spine-like tooth (Plate II. fig. 8, $1'$) a little to the outer side of its base.

The two outer series of the pleuræ (Plate II. fig. 8, 2, 3), as in all the other Heteropods, consist of simple tenaculiform hooks, varying in relative length, strength, and curvature, in the different species of the genus.

The configuration of the teeth, and the mode of action of the whole dental apparatus, are adapted for seizing and transfixing living prey; indeed, all the Heteropods are eminently carnivorous, and commonly bolt their victims whole. I have taken a *Firola*, which I recognised by its dentition, from the stomach of *Carinaria*. I also found a whole *Eurybia* impacted in the gullet of another specimen; and it is not an uncommon thing to meet with one of its own kind in the stomach of *Atlanta*, although it more usually preys upon the smaller *Pteropods*, *Spiralis*, or *Creseis*, for example.

From the so-called buccal mass (Plate II. fig. 1, h) a lengthy œsophagus courses directly backwards, and having entered the base of the visceral chamber, it soon opens into the anterior and inferior wall of a simple sub-globular stomach (Plate II. fig. 1, γ). This latter viscus lies a little below, and posterior to the heart (Plate II. fig. 1, y), and from its upper and fore part, a short intestine (Plate II. fig. 1, x) passes forwards above, and nearly parallel with, the œsophagus, and in close relation to the heart, to end in the vent (Plate II. fig. 1, s), deeply within the mantle cavity.

The liver (Plate II. fig. 1, ϵ) is a beautifully sacculated gland, of a pale-brown tint, often mottled with amber, black and red, commencing by a small pointed extremity near the nucleus of the shell, and gradually increasing in bulk, until it reaches near the posterior wall of the stomach, into which a single short and stout biliary duct opens.

I may notice here a conspicuous glandular body, which appears to me to be a renal organ (Plate II. fig. 1, u). This lies immediately above the rectum, and between the latter and the auricle of the heart, and is made up of densely clustered, minute, and rounded follicles, communicating freely with each other; but I cannot tell whether its very distinct oval outlet (u) opens into the heart, pericardium, or into the cavity of the mantle. Between the apparent position of this orifice and the arms there is a small prominence, bearing a well-defined pore (Plate II. fig. 1, t) that may communicate with it, or with an aquiferous system.

The Mantle, Respiratory, and Circulatory Systems.—The free border of the mantle (Plate II. fig. 1, n) is often beautifully fimbriated, and beset with cilia, both stationary and vibratile; and in the extended state, an angular fold of it (Plate II. fig. 1, o) occupies the dorsal slit of the shell, and extends for some little depth between the laminæ at the base of the keel.

The branchiæ consist of a variable number of clavate bodies (Plate II. fig. 1, p), in single longitudinal series floating from the dorsal wall or roof of the mantle; and to the left of these, but passing forwards more obliquely, there is a narrow, strongly ciliated band (Plate II. fig. 1, q), in all probability in some way connected with respiration, when the animal is retracted into its shell.

Large venous sinuses (Plate II. fig. 1, r), distinctly observable between the layers of the mantle, appear to convey the return blood from the gills to the auricle of the heart (Plate II. fig. 1, w). The extent of this chamber is very much less defined than that of the ventricle (Plate II. fig. 1, y), which communicates with it by a large opening, guarded by a couple of valves. The muscular bundles of the ventricular walls are also much stouter than those of the auricle, and form a close interlacement, in the interstices of which the lining and investing membranes seem to meet together.

The great vessel arises posteriorly from the apex of the ventricle, and forms a short dilated axis (Plate II. fig. 1, α), from which two arterial trunks originate; one of these (Plate II. fig. 1, β) passes backwards to supply the abdominal viscera, while the other (Plate II. fig. 1, z) runs forwards beside the œsophagus and the root of the swimming-plate, divides into the pedal and buccal arteries, in every respect conformable with those already described in *Firoloides*.

Generative System.—It is very remarkable that there is, as nearly as possible, an equal distribution of males and females in the genus *Firoloides*, while the proportion of males to females in *Atlanta* is so very great, as to render it difficult to form a correct estimate. In my own experience, out of many hundreds of *Atlantæ*, I have only met with about twenty females.

The follicles of the testicle (Plate II. fig. 1, ζ) somewhat resemble those of the liver, but they are at once distinguished by their lighter colour and the nature of their contents, which usually present a finely granular basis, with fasciculated striæ in the axis of every cavity and passage. The whole organ, though very similar in shape, is much smaller than the liver, between which and the inner wall of the shell-tube it lies. A stout *vas deferens* leads forwards from the base of the gland, and soon forms a fusiform enlargement (Plate II. fig. 1, δ) coated with black pigment; and from the small fore-part of which the duct still passes forwards a little way, and then terminates in a leaf-like expansion (Plate II. fig. 1, v), having a fine cellular structure in the space between the adductor-muscle (Plate II. fig. 1, η) and the rectum (Plate II. fig. 1, x).

The external male organ is situated at the base of the neck, and may be

represented as bifid, having an anterior part, conical and grooved, and a posterior apparently glandular and trumpet-like segment (Plate II. fig. 1, m). I have never been able to trace the *vas deferens* to this organ in any of the *Heteropods*; and having found the mantle-cavity full of spermatozoa in *Carinaria*, I am disposed to think that the scheme must be similar to that which obtains amongst ordinary gasteropods, when the penis is imperforate.

The ovary in the females is very similar to the testicle, both in shape and position, but the contained ova at once decide the difference, and the large thick-walled and convoluted oviduct, without the dark-coloured fusiform dilatation characteristic of the male sperm-duct, still further settles the question of sex. The aperture of the oviduct would appear to lie deep within the mantle. I could discover no opening in the position occupied by the penis in the male, though I have observed ova escape from beneath the margin of the mantle.*

The characters of the operculum are also significant,—viz., as to its general form, the course of the lines of growth, the position, size, and appearance of the nucleus, the muscular impressions, and its smooth or dotted surface.

Oxygyrus (Benson).

The genus *Oxygyrus*, established by Mr Benson, includes the *Atlantæ* of Rang, Kerandrien, and Lamanon, characterised by having an involute instead of a spiral nucleus to the shell, greater fulness in the whorls, and a comparatively small amount of calcareous matter as a component, particularly near the mouth and in the keel (Plate II. fig. 3).

The most striking difference that I have noticed between the soft anatomy of *Atlanta* and of *Oxygyrus* is that the testis is short and broad, lying at the base of the liver in the latter case; whereas it is much elongated in the former, extending far inwards between the liver and the columnellar wall of the shell.

The rachidian plates in all the *Oxygyri*, instead of a single dental process of *Atlanta*, bear three conical teeth on their posterior border; and these teeth are either all large and nearly equal in appearance (Plate II. fig. 5, e), or the middle tooth alone is well developed, while the lateral ones are rudimentary (Plate II. fig. 5, f).

The pleuræ are essentially similar to those of *Carinaria* and *Cardiapoda*, presently to be described, the most important character in all being the presence of a small conical, and often incurved tooth (Plate II. fig. 7, 1′), on a little shoulder

* All the species of *Atlanta* present so close a general resemblance, that their nice discrimination requires careful examination and comparison. The keel may be more or less, or not at all interposed between the peristome and the body whorl. It may be plain or wavy, though the shell never exhibits this latter character, as it does in *Carinaria*. The spire, as to its prominence, depression, evenness or obliquity, closeness or openness of its whorls, smoothness, dotted surface, or linear markings, affords us characters which do not appear to have been hitherto sufficiently recognised. There is, moreover, in colour, which one would naturally regard as being of little importance, a scarcely ever failing peculiarity of species.

at the inner side of the principal fang of the first or internal pleural series (Plate II. fig. 7, 1); whereas, if a smaller tooth is at all present in *Atlanta*, it is at a corresponding distance to the outer side of the larger one (Plate II. fig. 8, 1″).

Firola (Lesueur).

I have only met with one species of *Firola* (Plate I. fig. 7), which appears to be equally plentiful in the tropical parts of the two great oceans, Pacific and Atlantic. It is, I believe, *F. Lesueurii*, and about two inches long, or nearly the same size as *Carinaria Gaudichaudi* (Plate II. fig. 4). The body is elongated and cylindrical, with a full and rounded cephalic region, a muzzle reminding one of an elephant's trunk, and a laterally compressed rudder-like tail. The filamentous appendage in my first specimens was either absent or accidentally broken off, and the latter accident happened to the head of several examples of this species taken in the West Indies. No tentacula, or even frontal processes, were at all visible; but the eyes were well developed, and appeared to make a nearer approach to the eyes of *Carinaria* and *Cardiapoda* than do those of *Firoloides*.

The acoustic capsules may be readily recognised by the brilliancy of the contained otoliths, a little internal and posterior to the eyes; the auditory nerves being about equal to the optic in length, but very much more slender (Plate I. fig. 8, g).

A fan-like foot (Plate I. fig. 7, g) springs from about the middle of the under surface of the body, the rudimentary mesopodium (Plate I. fig. 7, f) being situated on the free margin of the organ, somewhat nearer its anterior than its posterior extremity.

The visceral nucleus (Plate I. fig. 7, k) occupies a deep notch on the dorsal surface of the body, near its hinder end, and is enveloped in a glistening fibrous coat, tinted with a madder-brown pigment. It is rather small, as compared with the size of the whole animal, but, being surmounted with distinct branchiæ, one more indication is afforded of the propriety of placing *Firola* between *Firoloides* and *Cardiapoda*; and this conclusion is strengthened by the position occupied by the sucker-disc of the foot, and also by the characters of the generative organs, which indicate something intermediate between the two genera alluded to.

The comparison of the lingual dentition of *Firoloides*, *Firola*, and *Cardiapoda*, will not only show how intimately they are related, but afford some assistance in determining their relative position with regard to the other *Heteropoda*.

The rachidian plates of *Firola* (Plate I. fig. 9, o; Plate II. figs. 5, 6) are quadrilateral in figure, but about three times broader than they are long; the dental points, as in *Firoloides*, form a broad comb, with a stout central fang; but the teeth gradually diminish in size towards the sides, and are so strongly marked, as compared with those of *Firoloides*, that it would be impossible to mistake the one for the other.

The first series of pleural plates very rarely exhibit a slight rudiment of the small internal tooth so characteristic of *Cardiapoda*, *Carinaria*, and *Oxygyrus*,

but the two lateral uncini present only the relative character of being comparatively long and slender.

Cardiapoda (D'Orb.)

Looking upon *Cardiapoda pedunculata*, *carinata*, *caudina*, and *placenta* (figured respectively in *D'Orb. Voy. Amer. Merid.*, t. 11, figs. 5, 3, and 4; and *Voy. la Bonite*, t. 17, f. 11, 1–5), as probably the same species, I am at a loss to know what I should call *my* species, which is evidently the same thing.

My first specimen, of which I immediately made a drawing (Plate I. fig. 10), was obtained in the S.W. Pacific, and scarcely exceeded $\frac{3}{4}$ inch in length. The muzzle was much fuller and more cylindrical than that of *Firoloides*, and the buccal mass equalled about one-third of its extent. The eyes were remarkably broad, and closely resembled those of *Carinaria* and *Oxygyrus*. They were fronted by simple conical tentacula. The auditory sacs were visible through the stout integument at some little distance behind the eyes.

The muscular sheath of the body exhibited a close and even tissue round the snout, head, and neck; but, on approaching the position of the foot, it formed itself into a number of longitudinal linear bundles, which passed, on the one hand, into the pedicle of the viscera, and, on the other, into the tail.

The propodial foot (Plate I. fig. 10, g) crested the middle of the ventral surface, and on its posterior border it bore the mesopodial disc (Plate I. fig. 10, h), which was laterally bilobed, and more highly developed than it is either in *Firoloides* or *Firola*.

The tail (*metapodium*) was rather remarkable in its appearance. Thus, being at first cylindrical, it soon exhibited a subterminal enlargement, from which again it suddenly tapered into a lengthy filiform appendage (Plate I. fig. 10, k). The enlargement just noticed was convex above, corresponding with the position of the operculum in *Atlanta* and *Oxygyrus*, while its inferior surface was expanded into a kind of disc, with a jagged, prominent ring-like border, densely coated with black pigment (Plate I. fig. 10, i). The use of this organ is yet unknown to me, but it appears to be homologous with the peculiar structure above described, as occurring on the under surface of the metapodium of *Atlanta*.

The whole extent of the visceral nucleus, shell, keel, and all, scarcely equalled the expansion of the foot; and the internal organs in general were so invested with pigmentary matter that the heart (Plate I. fig. 10, m) was the only one distinctly apparent at a cursory glance.

Numerous branches (Plate I. fig. 10, n), with a plain external surface and a zig-zag internal fold, protruded from beneath the dorsal lip of the shell, which was semicartilaginous, shallow, or scoop-shaped, with an involute nucleus, and a deep but very thin and delicate keel.

The external male organ (Plate I. fig. 10, o) resembled that of *Carinaria*, con-

sisting of an interior grooved, and a posterior glandular portion,—the latter being homologous with the trailing bulb of *Firoloides*, and, I believe, also with the trumpet-like segment of *Atlanta*.

The rachidian plates of *Cardiapoda* (Plate I. fig. 11, o; Plate II. fig. 5, c), are broad and slightly concave in front, but rather more so behind, with the angles obtuse in front, and sharp and incurved posteriorly. The dental processes are only three in number,—the central one being large and broadly conical, and the lateral ones very small.

The little tooth on the inner side of the principal fang of the first pleural series is distinctly developed, but the uncini do not appear to differ in any essential particular from those of *Firola*.

Carinaria Gaudichaudi (Plate II. Figure 4.)

This species I found to have as wide a range as the *Firola* and *Firoloides* previously described. Its length varies from $1\frac{1}{2}$ to 2 inches, and altogether it looks like an *Atlanta* whose head and body had much outgrown the capacity of its shell. On comparing the shells of the two genera, they will be found to resemble one another in several particulars. Thus, they are both dextrally-spiral, with a distinct umbilicus in the axis of the spire, and a prominent keel on the dorsal border of the whorls; but both shell and keel in *Carinaria* are beautifully crimped transversely, and the last whorl increases very rapidly in keeping with the development of the animal, so that the mouth of the full grown shell is widely separated from its spiral nucleus.

The body is pellucid and colourless, but slightly, or not at all, tuberculated. The inner surface of the integument, however, is studded at pretty equal distances with little clusters of cells like those of *Firoloides*. M. Rang believed that a tuberculated epidermis was always present in *Carinaria*, forming a distinguishing feature between it and *Firola*, in which, according to him, the outer integument is always smooth (?) He makes allusion evidently to the little clusters of cells above noticed as the representatives in *Firola* of the tuberculations of *Carinaria*.

The proboscis is abruptly truncated at its extremity, and very variable as to its length and fulness. The eyes are fronted by small tentacula, and the acoustic sacs, as in *Cardiapoda*, &c., are appended to long and delicate auditory nerves. The ciliated lining of the sacs in this species I have been enabled to observe more distinctly than in any of the other Heteropods described above.

The abdominal fin is fan-shaped, with a thin transparent margin; and the sucker-disc is represented by a little cup-like dilamination of its posterior border. The tail, or metapodium, is laterally compressed, and tapers to a point, without supporting an obvious rudder-fin, like that commonly given in figures of *Carinaria mediterranea*.

The visceral mass under protection of the shell is elevated, as it were, upon a short pedicle springing from the dorsal region, at a point considerably posterior to the swimming-fin.

A row of reddish-tinted branchiæ protrude beyond the mantle margin, and, as in *Cardiapoda*, the heart and intestine are distinctly visible through the shell (Plate II. fig. 4′, b, d).

The rachidian plates of *Carinaria* (Plate II. fig. 5, d, and fig. 6, o) are not unlike those of *Cardiapoda*, but the dental processes in each are perfectly characteristic. Thus, *Carinaria* is distinguished by three large subequal teeth, flanked on each side by a rudimentary tubercle.

The small internal tooth of the first pleural series is more highly developed in *Carinaria* than in *Oxygyrus*; and all the members of the pleuræ (Plate II. fig. 6, 1, 2, 3.), are much stouter in the former than in the latter genus, though they are somewhat exceeded in this respect by those of *Cardiapoda* (Plate I. fig. 11, 1, 2, 3.)

REFERENCES TO THE FIGURES.

PLATE I.

Fig. 1. Firoloides.

a. Tentacula.
b. Eyes.
c. Auditory sac.
d. Buccal mass.
e. Œsophagus.
f. Sucker-disc.
g. Swimming fin.
h. Pedal ganglia.
i. Heart.
k. Rectum.
l. Vas deferens.
m. Bulbous portion of the penis.
n. Caudal appendage.

Fig. 2. *Head and Muzzle of* Firoloides.

a. Mouth.
b. Lingual sac.
c. Salivary glands.
d. Buccal ganglia.
e. Buccal artery.
f. Buccal nerves.
g. Œsophagus.
h. Optic nerves.
i. Cerebroid ganglia.
k. Tentacles.
l. Eyes.
m. Auditory sacs.
n. Nerve trunks connecting *i* with the pedal ganglia.
o. Muscular sheath.

Fig. 3. *Pedal Ganglia and Great Vessels of* Firoloides.

a. Pedal ganglia blended together by their contiguous surfaces, and giving off nerves to the foot and neighbouring parts.
b. Anterior division of aorta.
c. Pedal artery.
d. Buccal artery.

Fig. 4. *Visceral Mass, &c., of* Firoloides (*female*).

a. Great nerve supplying branches to the tail, and connecting the pedal with the visceral ganglia.
b. Anterior division of the aorta.
c. Œsophagus.
d. Cardiac ganglion ?
e. Principal visceral ganglion, sending a conspicuous nerve to the mouth and its sphincter muscle.
f. Liver.
g. Metapodium.
h. Caudal appendage.
i. Vagina.
k. Small leaflets or lobes on the sides of the ♀ opening.
l. Oviduct.
m. Flexure of *l*.
n. Oval opening of the mouth.
o. Anal aperture in front of a ciliated papilla.
p. And between, two little leaflets like *k*.
q. Ciliated elevation, with depressed centre, probably a respiratory organ.
r. Little clusters of cells, noticed at p. 8.
s. Auricle of the heart.
t. Ventricle.
u. Muscular sheath.

Fig. 5. *Lingual Dentition of* Firoloides.

o. Rachis.
1–3. Pleuræ.

Fig. 6. *Nidamental Chord of* Firoloides.

Fig. 7. Firola (*considerably enlarged*).

a. Buccal mass.
b. Œsophagus.
c. Eye.
d. Auditory sac.
e. Pedal ganglia.
f. Mesopodium.
g. Swimming-fin.
h. Penis.
i. Branchiæ.
k. Visceral mass.
l. Metapodium.

Fig. 8. *Brain and Organs of Sense of* Firola.

a. Upper cerebroids, sending off anterior and posterior branches.
b. Lower cerebroids, giving off motor nerves to the eyes.
c. Buccal nerves.
d. Motor nerve of the eye.
e. Optic nerves.
f. Trunks communicating with the pedal ganglia.
g. Auditory nerve.
h. Acoustic sacs.
i. Lens.
k. Body of the eye.
l. Retina.

Fig. 9. *Lingual Dentition of* Firola Lesueuri.

o. Rachis.
1–3. Pleuræ.

Fig. 10. Cardiapoda *in motion*.

a. Buccal mass.
b. Œsophagus.
c. Tentacula.
d. Eyes.
e. Ear-sacs.
f. Pedal ganglia.
g. Swimming-plate.
h. Sucker-disc.
i. Curious structure noticed at p. 17.
k. Caudal process.
l. Visceral mass.
m. Heart.
n. Branchiæ.
o. Penis.

Fig. 11. *Lingual Dentition of* Cardiapoda.

o. Rachis.
1–3. Pleuræ.
1′ Small inner tooth of first pleural series.

PLATE II.

Fig. 1. Atlanta.

a. Mouth.
b. Buccal mass.
c. Tentacula.
d. Tentacular (?) tubercle at the inner and fore part of the eye.
e. Eyes.
f. Cerebroid ganglia.
g. Ear-sac.
h. Salivary glands.
i. Pedal ganglia.
j. Propodium.
k. Mesopodium.
l. Œsophagus.
m. Penis.
n. Mouth margin.
o. Carinal fold of *n*.
p. Branchiæ.
q. Ciliated line.
r. Venous sinuses.
s. Vent.
t. Aquiferous pore?
u. Glandular organ, probably renal.
v. Expanded orifice of the sperm-duct.
w. Auricle of the heart.
x. Intestine.
y. Ventricle.
z. Anterior division of aorta.
α. Aortic axis.
β. Visceral artery.
γ. Stomach.
δ. Dilatation of sperm-duct.
ε. Liver.
ζ. Testicle.
η. Origin of retractor muscle.
θ. Metapodium.
ι. Tongue-like process.
κ. Fossa.
λ. Operculum.
μ. Vertical fold.

Fig. 1′. *Operculum of* 1, *magnified*.

Fig. 2. Oxygyrus.

a. Buccal mass and muzzle.
b. Tentacula.
c. Eyes.
d. Propodium.
e. Mesopodium.
f. Metapodium.
g. Operculum.

Fig. 2′. *Operculum of* 2, *magnified*.

Fig. 3. Oxygyrus, *n. s.*

References as in Fig. 2.

Fig. 4. Carinaria.

a. Buccal mass.
b. Tentacula.
c. Eyes.
d. Ear-sacs.
e. Impacted food in the gullet.
f. Pedal ganglia.
g. Propodium.
h. Mesopodium.
i. Metapodium.
k. Penis.
l. Branchiæ.
m. Visceral mass.

Fig. 4′. *Shell and Viscera magnified*.

a. Gullet.
b. Rectum.
c. Branchiæ.
d. Heart.

Fig. 5. *The Rachidian Plates characteristic of the genera of* Heteropoda.

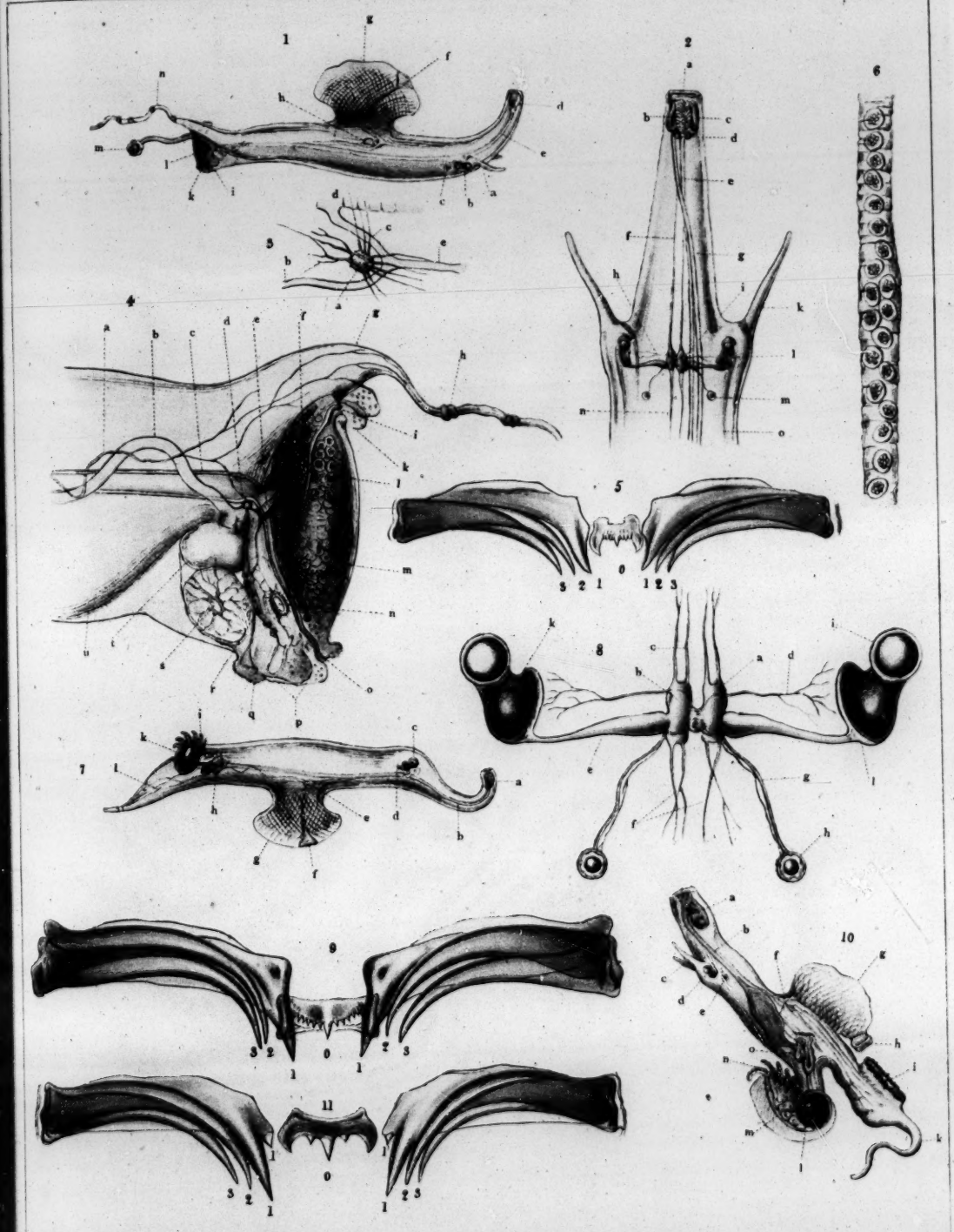
a. Firoloides.
b. Firola
c. Cardiapoda.
d. Carinaria.
e and *f*. Two divisions of Oxygyrus, probably distinct genera.
g. Atlanta.

Fig. 6. *Lingual Dentition of* Carinaria.

Fig. 7. *Do.* *do.* Cardiapoda.

Fig. 8. *Do.* *do.* Atlanta.

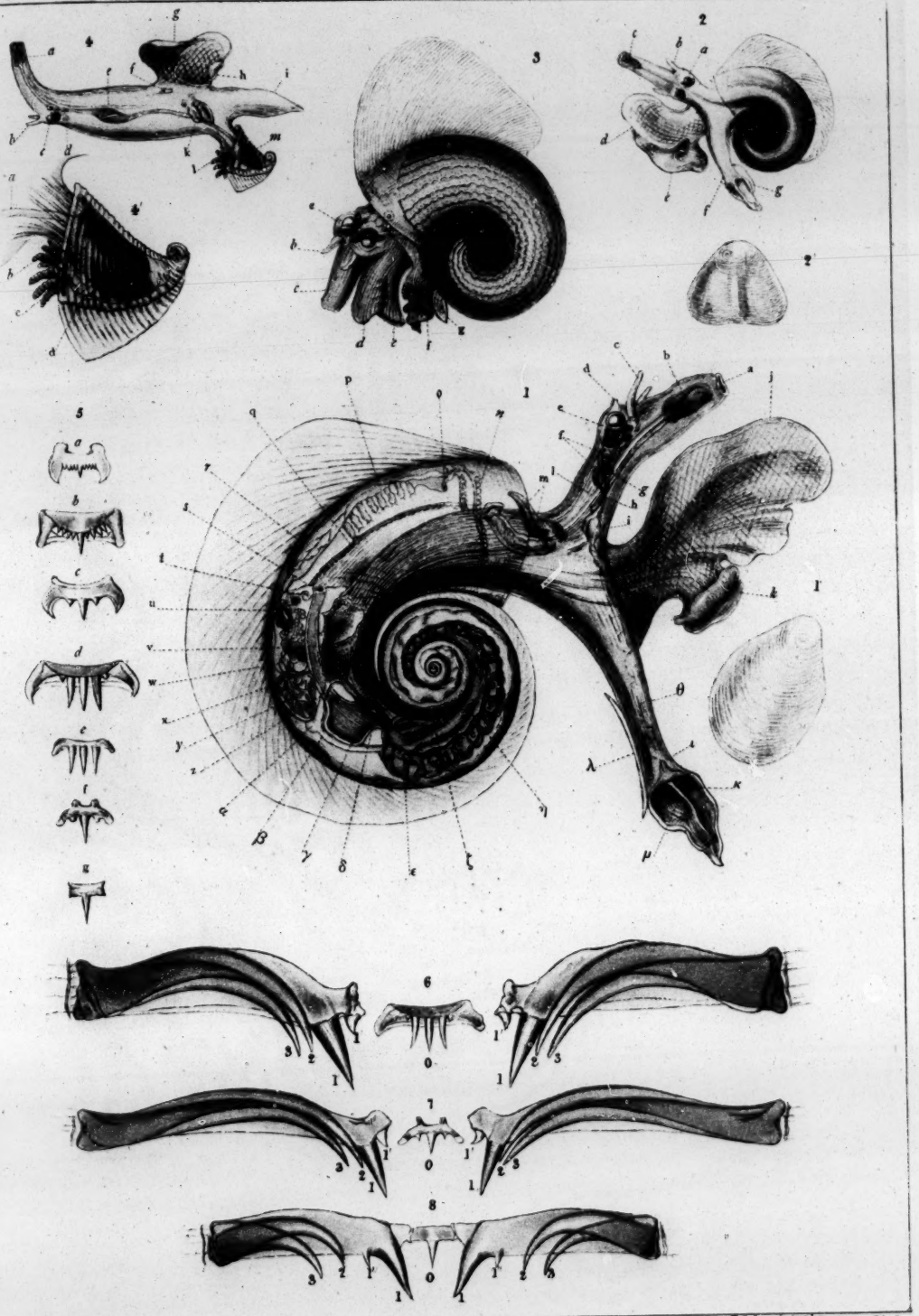
o. Rachis
1–3. Pleuræ.
1′. Tooth common to Cardiapoda, Carinaria, and Oxygyrus.
1″. Tooth characteristic of Atlanta.



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II.—*Investigation of an Expression for the Mean Temperature of a Stratum of Soil, in Terms of the Time of Year.* By Joseph D. Everett, M.A., Professor of Mathematics, &c., in King's College, Windsor, Nova Scotia.*

(Read 3d February 1862).

1. It is a well-known property of simple harmonic functions, that the sum of any two or more of them having the same period, is itself a simple harmonic function having the same period as its components. The same thing must be true of their mean, since this is equal to the sum divided by a constant; and it will still be true when the number of components is indefinitely great, and the mean becomes an integral.

2. Let v denote a variation of temperature which is a simple harmonic function of the time t, so that

$$v = \mathrm{A} \sin (nt + \mathrm{E}),$$

where t is expressed in arc at the rate of an entire circumference to the year. The mean value of v for any assumed interval of time will be

$$\frac{\int v\,dt}{\int dt}$$

taken between limits corresponding to the beginning and end of the interval. Performing the operations indicated, it will be found that the expression for the mean temperature of an interval equal to the $\frac{1}{m}$th part of a year, t being the time for the centre of the interval, is

$$\mathrm{A}\frac{\sin\frac{n\pi}{m}}{\frac{n\pi}{m}} \sin (nt + \mathrm{E}).$$

3. If v in last section represent a variation of temperature at depth x, below the surface of the ground, then, by the theory of underground conduction, A and E are functions of x. Hence the mean value v at time t for a stratum of soil will be

$$\frac{\int v\,dx}{\int dx}$$

taken between limits, corresponding to the top and bottom of the stratum. The

* See article by the Author, in the Edinburgh New Phil. Journal, vol. xvi., 1861.

soil is here supposed to be uniform as regards its thermal qualities; and its surface, as also the bounding planes of the stratum, horizontal.

4. We propose to effect the integration above indicated, and deduce convenient formulæ for the coefficients in the expression, for the mean temperature of a stratum in terms of the time. The data to be supplied by observation are the temperatures at all times of the year, at two arbitrarily assumed depths. The observed temperatures must be reduced to harmonic expressions (one for each depth), and from these it is easy to deduce the harmonic expression for the temperature at any third depth. We may therefore assume that the harmonic expression for the temperature at the centre of the stratum is known; and we shall find it convenient to compare the amplitudes and epochs in this expression with those in the expression for the mean temperature of the stratum. Let the former expression be

$$\bar{v} = A_0 + A_1 \sin(t + E_1) + A_2 \sin(2t + E_2) + \&c.$$

and the latter

$$V = A_0 + m_1 A_1 \sin(t + E_1 + \alpha_1) + m_2 A_2 \sin(2t + E_2 + \alpha_2) + \&c.$$

We have to find the values of the constants m_1 m_2, &c., which express the ratios of the amplitudes in the two series, and of α_1 α_2, &c., which expresses the differences of epoch.

5. By the theory of underground conduction, the temperature at depth x below the centre of the stratum will be

$$v = A_0 + A_1 e^{-x\sqrt{\frac{\pi c}{k}}} \cdot \sin\left(t + E_1 - x\sqrt{\frac{\pi c}{k}}\right) + A_2 e^{-x\sqrt{\frac{2\pi c}{k}}} \cdot \sin\left(2t + E_2 - x\sqrt{\frac{2\pi c}{k}}\right) + \&c.$$

the general term being

$$+ A_n e^{-x\sqrt{\frac{n\pi c}{k}}} \cdot \sin\left(nt + E_n - x\sqrt{\frac{n\pi c}{k}}\right)$$

e denoting the base of Napierian logarithms, π the ratio of circumference to diameter, k the conductivity of the soil referred to unit volume, and c the capacity for heat similarly referred.

6. We will first suppose that the expression for the temperature at the centre of the stratum is simply

$$\bar{v} = \sin t.$$

Then we shall have

$$v = e^{-x\sqrt{\frac{\pi c}{k}}} \cdot \sin\left(t - x\sqrt{\frac{\pi c}{k}}\right)$$

and if ξ be the thickness of the stratum, the mean temperature at time t will be

$$\frac{1}{\xi}\int v\,dx$$

taken between $-\frac{1}{2}\xi$ and $+\frac{1}{2}\xi$.

Now
$$\int v dx = \int e^{-x\sqrt{\frac{\pi c}{k}}} \sin\left(t - x\sqrt{\frac{\pi c}{k}}\right) dx$$

$$= e^{-x\sqrt{\frac{\pi c}{k}}} \frac{\cos\left(t - x\sqrt{\frac{\pi c}{k}}\right) - \sin\left(t - x\sqrt{\frac{\pi c}{k}}\right)}{2\sqrt{\frac{\pi c}{k}}}$$

and using z to denote $\frac{1}{2}\xi\sqrt{\frac{\pi c}{k}}$ we shall obtain as the value of $\frac{1}{\xi}\int v dx$ between the given limits, the expression

$$V = \frac{1}{4z}\left(e^z + e^{-z}\right)\sin z\,(\sin t + \cos t)$$
$$+ \frac{1}{4z}\left(e^z - e^{-z}\right)\cos z\,(\cos t + \sin t)$$
$$= \frac{1}{2z\sqrt{2}}\left(e^z + e^{-z}\right)\sin z \cos\left(t - \frac{\pi}{4}\right)$$
$$+ \frac{1}{2z\sqrt{2}}\left(e^z - e^{-z}\right)\cos z \sin\left(t - \frac{\pi}{4}\right).$$

Putting
$$\left(e^z + e^{-z}\right)\sin z = P \;.\; \left(e^z - e^{-z}\right)\cos z = Q,$$

we have
$$V = \frac{P}{2z\sqrt{2}} \cdot \cos\left(t - \frac{\pi}{4}\right) + \frac{Q}{2z\sqrt{2}} \cdot \sin\left(t - \frac{\pi}{4}\right)$$

a simple harmonic function of t, which can be reduced to the form
$$V = m \;.\; \sin(t + \alpha)$$

by making
$$\tan\left(\frac{\pi}{4} + \alpha\right) = \frac{P}{Q} \text{ and } m = \frac{1}{2z\sqrt{2}} \cdot \sqrt{P^2 + Q^2} = \frac{1}{2z\sqrt{2}} \cdot Q \sec\left(\frac{\pi}{4} + \alpha\right).$$

7. The values of m and α can be found by the formulæ of last section; but more convenient formulæ when z is small are obtained by developing in ascending powers of z. For the value of m we have

$$m^2 = \frac{1}{8z^2}(P^2 + Q^2) = \frac{1}{8z^2}\left(e^{2z} + e^{-z} - 2\cos 2z\right)$$
$$= \frac{1}{4z^2}\left(1 - \frac{(2z)^2}{1\cdot 2} + \frac{(2z)^4}{\underline{|4}} + \&c.\right) - \frac{1}{4z^2}\left(1 - \frac{(2z)^2}{1\cdot 2} + \frac{(2z)^4}{\underline{|4}} - \&c.\right)$$
$$= \frac{1}{2z^2}\left(\frac{(2z)^2}{1\cdot 2} + \frac{(2z)^6}{\underline{|6}} + \frac{(2z)^{10}}{\underline{|10}} + \&c.\right)$$
$$= 1 + \frac{2}{45}z^4 + \frac{2}{14175}z^8 + \&c.,$$

a series which converges with extraordinary rapidity, and may be used even for large values of z. Extracting the square root, we have

$$m=1+\frac{1}{45}z^4-\frac{1}{5670}z^8+\&c.$$

8. For the value of the acceleration α we have

$$\tan\left(\frac{\pi}{4}+\alpha\right)=\frac{P}{Q}=\frac{\left(e^z+e^{-z}\right)\cdot\sin z}{\left(e^z-e^{-z}\right)\cdot\cos z}$$

$$=\frac{\left(1+\frac{z^2}{1\cdot2}+\frac{z^4}{\underline{4}}+\&c.\right)\left(1-\frac{z^2}{\underline{3}}+\frac{z^4}{\underline{5}}-\&c.\right)}{\left(1+\frac{z^2}{\underline{3}}+\frac{z^4}{\underline{5}}+\&c.\right)\left(1-\frac{z^2}{1\cdot2}+\frac{z^4}{\underline{4}}-\&c.\right)}$$

Now, the first factor of the numerator is obviously greater than the first factor of the denominator, and it may be shown that the second factor of the numerator exceeds the second factor of the denominator by the quantity,—

$$\frac{z^2}{1\cdot3}-\frac{z^4}{\underline{3}\cdot5}+\frac{z^6}{\underline{5}\cdot7}-\frac{z^8}{\underline{7}\cdot9}+\&c.,$$

which is positive for all values of z not greater than unity. Hence, for all such values of z, $\tan\left(\frac{\pi}{4}+\alpha\right)$ is greater than unity, and therefore α is positive; that is to say, the phases of temperature are earlier for the mean than for the centre of the stratum.

9. In the expression $\frac{e^z+e^{-z}}{e^z-e^{-z}}\tan z$, the first factor is always finite and positive for finite and positive values of z. Hence, when $\tan z$ is zero or infinity, $\tan\left(\frac{\pi}{4}+\alpha\right)$ is also zero or infinity. And we know from general considerations that when $z=0$, $\alpha=0$. Hence it may be shown, that when z is of the form $\frac{i\pi}{2}$, where i is any integer (not including zero), $\frac{\pi}{4}+\alpha=z$, or $\alpha=z-\frac{\pi}{4}$. For all other values of z, $\frac{\pi}{4}+\alpha$ will be unequal to z, since the factor $\frac{e^z+e^{-z}}{e^z-e^{-z}}$ is never equal to unity.

10. We have hitherto been supposing the expression for the temperature at the centre to be,—

$$v=\sin t.$$

To render our results applicable to the general term

$$A\sin(nt+E),$$

in the expression for the temperature at the centre (§ 3), and the corresponding term

$$mA\sin(nt+E+\alpha)$$

in the expression for the mean temperature, we have merely to substitute $z\sqrt{n}$ for z, as will readily appear upon trial. Making this substitution, we have

$$P=\left(e^{z\sqrt{n}}+e^{-z\sqrt{n}}\right)\sin z\sqrt{n},\ Q=\left(e^{z\sqrt{n}}-e^{-z\sqrt{n}}\right)\cos z\sqrt{n},$$

$$\tan\left(\frac{\pi}{4}+\alpha\right)=\frac{P}{Q},\ m=\frac{1}{2z\sqrt{2n}}\cdot Q\sec\left(\frac{\pi}{4}+\alpha\right) \quad . \quad . \quad . \quad (1.)$$

$$m=\sqrt{\left(1+\frac{2}{45}n^2z^4+\frac{2}{14175}n^4z^8+\&c.\right)} \quad . \quad . \quad (2.)$$

$$=1+\frac{1}{45}n^2z^4-\frac{1}{5670}n^4z^8+\&c., \quad . \quad . \quad . \quad . \quad (3.)$$

$$\tan\left(\frac{\pi}{4}+\alpha\right)=\frac{e^{z\sqrt{n}}+e^{-z\sqrt{n}}}{e^{z\sqrt{n}}-e^{-z\sqrt{n}}}\cdot\tan z\sqrt{n}$$

$$=\frac{e^{2z\sqrt{n}}+1}{e^{2z\sqrt{n}}-1}\tan z\sqrt{n}=\frac{1+e^{-2z\sqrt{n}}}{1-e^{-2z\sqrt{n}}}\cdot\tan z\sqrt{n}, \quad . \quad . \quad (4.)$$

Assume $\tan\theta=e^{-2z\sqrt{n}}$

then $$\tan\left(\frac{\pi}{4}+\alpha\right)=\tan\left(\frac{\pi}{4}+\theta\right)\cdot\tan z\sqrt{n}, \quad . \quad . \quad . \quad (5.)$$

Or assume $\tan\varphi=e^{-z\sqrt{n}}$

then $$\tan\left(\frac{\pi}{4}+\alpha\right)=\sec 2\varphi\cdot\tan z\sqrt{n}, \quad . \quad . \quad . \quad . \quad (6.)$$

Also, from (§ 8.)

$$\tan\left(\frac{\pi}{4}+\alpha\right)=\frac{1+\frac{1}{3}nz^2-\frac{1}{30}n^2z^4-\&c.}{1-\frac{1}{3}nz^2-\frac{1}{30}n^2z^4+\&c.}$$

$$=1+\frac{2}{3}nz^2+\frac{2}{9}n^2z^4+\&c. \quad . \quad . \quad . \quad (7.)$$

whence

$$\tan\alpha=\frac{\frac{2}{3}nz^2+\frac{2}{9}n^2z^4+\&c.}{2+\frac{2}{3}nz^2+\frac{2}{9}n^2z^4+\&c.}$$

$$=\frac{1}{3}nz^2+\&c. \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

the next term being $+\frac{1}{105}n^3z^6$.

A good approximation to the value of m will generally be obtained by assuming

$$\tan\omega=nz^2\sqrt{\frac{2}{45}}$$

and putting $m=\sec.\omega.$

11. From equations (3) and (8), we learn, that when nz^2 is small, the in-

crease of amplitude is nearly proportional to the fourth power, and the acceleration to the square of z; and z is by hypothesis proportional to the thickness of the stratum being equal to half the thickness multiplied by $\sqrt{\frac{\pi c}{k}}$. It is important to observe, that the values of m and α are entirely independent of the depth of the stratum below the surface of the ground.

The approximate values of m and tan α are—

$$m = 1 + \frac{1}{45} n^2 z^4, \quad \tan \alpha = \frac{1}{3} n z^2;$$

whence also, approximately,*

$$\log_e m = \frac{1}{45} n^2 z^4 \qquad \alpha = \frac{1}{3} n z^2.$$

These two last expressions are useful in estimating the corrections due to length of bulb, in the reduction of observations made with underground thermometers. Both the corrections are to be applied subtractively, the former to Napierian logarithm of amplitude, and the latter to epoch in circular measure.

12. At Calton Hill, Edinburgh, where underground temperatures have been observed, the value of $\sqrt{\frac{\pi c}{k}}$ is ·1156, the unit of measure employed being the French foot; hence for a stratum 2 French feet thick $z = ·1156$, and for a stratum 1 French foot thick $z = ·0578$. The corresponding values of $\log_e m$ and α for the annual and half-yearly terms are given below:—

		Log$_e$ m.	α in Circular Measure.
Annual term,	2 feet stratum, . . .	·00000397	·00445
... ...	1	·00000025	·00111
Half-yearly term,	2	·0000159	·00891
... ...	1	·0000010	·00223

13. For strata 24 and 12 French feet thick, the values of m and α are as under:—

	Values of m.		Values of α.	
	Annual Term.	Half-yearly Term.	Annual Term.	Half-yearly Term.
24 feet Stratum, . .	1·08005	1·2999	35° 42′	23° 23′
12 feet Stratum, . .	1·0051	1·0204	9 10	18 14

* More nearly,

$$\log_e m = \frac{1}{45} n^2 z^4 - \frac{2}{4725} n^4 z^8$$

$$\alpha = \frac{1}{3} n z^2 - \frac{8}{2835} n^3 z^6.$$

The annual term in the expression for the temperature at the depth of 12 feet in the same locality, as derived from direct observation, is—

$$2{\cdot}31 \sin (t + 179^\circ\ 15')$$

Hence the annual term in the expression for the mean temperature of the first 24 feet will be—

$$2{\cdot}31 \times 1{\cdot}08 \sin (t + 179^\circ\ 15' + 35^\circ\ 42')$$
$$= 2{\cdot}50 \sin (t + 214^\circ\ 57').$$

14. To correct for difference between temperature of bulb and that of stem, the following method may be adopted:—

Let the observed temperatures be represented by the expression

$$V = A_0 + P_1 \cos t + Q_1 \sin t + P_2 \cos 2t + Q_2 \sin 2t,$$

and the mean temperature of the stem by the expression

$$v = a_0 + p_1 \cos t + q_1 \sin t + p_2 \cos 2t + q_2 \sin 2t.$$

Then if r denote the ratio of the capacity of the stem to that of the bulb, and U the corrected value of V,

we have $U + rv = (1 + r)\,V$, or $U = V + r\,(V - v)$.

Hence the correction for A_0 is . . . . $r(A_0 - a_0)$

... ... P_1 is . . . . $r(P_1 - p_1)$

... ... Q_1 is . . . . $r(Q_1 - q_1)$

and so on for the other terms.

A different mode of correction is described in Principal FORBES's paper, "Account of some Experiments on the Temperature of the Earth," &c., Trans. Roy. Soc. Edin., Vol. XVI. Part II.

15. For the theory of underground conduction, and the use of harmonic functions in connection therewith, reference may be made to two papers on "Underground Temperature," by Professor W. THOMSON and the Author, Trans. Roy. Soc. Edin. ,Vol. XXII. Part II. (April 1860).

III.—*On a Difficulty in the Theory of Rain.* By James Dalmahoy, Esq.

(Read 7th April 1862.)

Nearly a hundred years ago,* Dr Heberden of London made the following experiment:—Having prepared three exactly similar rain-gauges, he placed one of them on the roof of Westminster Abbey, another on the roof of a neighbouring house, but at a much lower level, and the third in the garden of the same house. At the end of twelve months he found that the gauge on the roof of the Abbey had received 12·099 inches of rain, the gauge on the neighbouring house-top 18·139 inches, and the gauge on the ground 22·608 inches.

This paradoxical result required, of course, to be confirmed by other observers, and in other localities; and the similar results obtained by Dobson, Dalton, Howard, and especially by Arago at the Paris Observatory, and by Phillips at York, have amply supplied all that was wanting in this respect.

It may be noted in passing, as a curious fact, that in Dr James Hutton's "Theory of Rain" there is no allusion to Dr Heberden's observations, though these were published in the "Philosophical Transactions"† fourteen years before Dr Hutton's Theory was read to the Royal Society of Edinburgh.‡

Of the attempts which have been made to reconcile Dr Heberden's observations with facts and principles already established, that of Dr Franklin is the most plausible, and it has been very generally accepted as the true explanation. At first sight, indeed, it seems capable of explaining every difficulty; and it is only when more carefully examined, and especially when tested quantitatively by actual results, that its inadequacy becomes apparent.

As, however, many may be disposed to question this conclusion, I am glad to be able to rest the proof of it on the following quotation from Sir John Herschel's treatise on Meteorology.§

Having alluded to the fact that, during the year 1833–34, the quantity of rain received on the top of York Minster, at the height of 213 feet, bore to that received on the neighbouring ground the ratio of 1 : 1·706, the learned author proceeds as follows:—"The usual account given of this phenomenon (Koemtz) is, that rain falling from a high level, and therefore colder than air at the surface of the ground, arriving in an atmosphere nearly or quite saturated with moisture, condenses on itself, or causes the condensation, in the chilled air, of an additional

* 1766–67. † 1770. ‡ 1784.

§ Encyclopædia Britannica, 8th edition, article *Meteorology*, par. 109.

quantity of vapour. But it is evident that this cause, though not uninfluential, is totally inadequate to account for so great a difference. Admitting a given weight of rain to arrive at 213 feet from the ground, with the temperature of the region at which it was formed unaltered, and supposing it to acquire, in the remaining 213 feet, the full temperature of the air (both of them extreme and even extravagant suppositions), admitting, too (though hardly less extravagant), the mean height of the formation of rain to be 12,000 feet, it would bring down with it a cold of 40° Fahr., which would condense (whether on the drops or in saturated air, if diffused through it) only $\frac{40}{960} = \frac{1}{24} = 0{\cdot}42$ of its weight, = one-seventeenth of the quantity to be accounted for."

Although this demonstration does not really admit of being strengthened, yet, as showing how a similar conclusion was reached in a different manner, I beg to refer to a short paper in the 20th volume of the "Transactions of the Royal Society of Edinburgh," entitled, "On the Weight of Aqueous Vapour which is Condensed on a Cold Surface under given conditions," in which I have endeavoured to prove experimentally that, taking the observations at York during the three winter months of the years 1832–33, 1833–34, 1834–35, the increment which the rain received in falling between the level of the top of the Museum and the ground, a height of 44 feet, was above 600 times greater than it would have been if the condensation of aqueous vapour by cold had been the only cause in operation.

For these reasons, therefore, it seems necessary to reject this explanation, though one of such likelihood as to have suggested itself, independently, to Dr FRANKLIN, M. BOISGIRAUD, and Professor PHILLIPS.

The eminent meteorologist LUKE HOWARD proposed* an explanation which, however, seems to differ from the one just considered chiefly in that it supposes the cold, on which the condensation of vapour depends, to originate, in some unexplained way, in the atmosphere itself, instead of being brought down by the rain from the upper regions.

There is another mode of accounting for the difficulty under consideration, which has been advocated by Mr JEVONS† and other writers.‡ According to this theory, the deficiency of rain in the upper gauge is produced by wind—the gauge itself, or the building on which it stands, giving rise to eddies which partially obstruct the entrance of the rain into the mouth of the gauge.

That wind may affect the indications of a rain-gauge, has been proved by the interesting experiments of Professor A. D. BACHE of Philadelphia.§ Having placed gauges at the four angles of a high square tower, at a height of ten inches

* Report of British Association for 1834, p. 563.
† Philosophical Magazine for December 1861, p. 421.
‡ Dr STARK in Transactions of the Royal Scottish Society of Arts, vol. v. p. 66.
§ Report of British Association for 1838, Transactions of the Sections, p. 25.

above the parapet, he found that the gauges at the lee side of the tower received more rain than those at the windward side; but that, the same arrangement continuing, when gauges were also placed on poles at the height of six feet above the parapet, there was observed scarcely any difference between their indications.

The bearing which these results have on the present question will afterwards be noticed; but neither these, nor any similar facts which writers have adduced, seem to meet the special difficulty to be explained—namely, that of three gauges, at three different levels, and each variously placed as respects surrounding objects, the lowest gauge always receives more rain than the middle one, and the middle gauge more than the upper one.

But that which seems an unanswerable objection to this mode of explanation, is the fact that, when the upper and lower gauges have been inspected after a perfect calm, and when the rain fell perpendicularly, the upper gauge was still found to contain less rain than the lower one. This fact is recorded very expressly by ARAGO,* and also by PHILLIPS.†

While, therefore, it is denied, for the reasons now adduced, that the difficulty under discussion can be accounted for by the effect of wind, it is not disputed that, under certain circumstances, wind does modify the indications of a rain-gauge.

Sir JOHN HERSCHEL concludes his notice of the attempts which have been made to account for the phenomenon, in these words:‡—"The real cause is yet to seek, and there is no more interesting problem which can fix the attention of the meteorologist. Visible cloud rests on the soil at low altitudes above the sea-level but rarely, and from such cloud only would it seem possible that so large an accession of rain should arise."

I was first led to think of this puzzling question a good many years ago, and the result of my repeated attempts to find a solution of it is contained in the following hypothesis, which, in spite of its many defects, I venture humbly to submit to the consideration of those who take an interest in meteorology.

The hypothesis begins by taking for granted the truth of Dr HUTTON'S "Theory of Rain."§ It assumes that the spherules of water composing the clouds from which rain proceeds are, at their first formation, so small, that the terminal velocity of their descent is almost insensible. It further assumes that these minute globules coalesce, at innumerable points, into drops of sensible magnitude, and fall in the shape of rain; while portions of the cloud, which do not thus coalesce, are floated downward in a current of air, and fill the whole space between the clouds and the earth with minute particles of water.

This medium, consisting of cloud carded out, as it were, by the downward

* Œuvres, tome xii. pp. 409, 416.

† Report of British Association for 1833. See Transactions of Sections, pp. 403, 404. Report for 1834, p. 561.

‡ Article *Meteorology*, par. 109.

§ Transactions of Royal Society of Edinburgh, vol. i. p. 41.

current of air, and dispersed through a very large space, is assumed to be so rare as not to affect the transparency of the atmosphere; and it is the constant floating down of this medium in a current of air which, according to the hypothesis, is the principal and almost sole cause of the phenomenon to be accounted for.

But it will be necessary, at this point, to answer a question which may naturally be anticipated,—namely, whether there be any proof, from theory or observation, that rain is actually accompanied by a downward current of air and floating moisture.

Theoretically, two causes may be assigned for such a current. The first is the continual displacement of the air by the downward motion of the drops of rain; for, the effect of bodies in rapid motion to draw after them any light matter in their neighbourhood, such as air or smoke, is familiar to everybody; and that the drops of rain should have this effect will not seem improbable, when it is remembered that the largest of them may attain a velocity of nearly 400 inches per second.

The second cause of the downward current of air which theory suggests, is the cold which rain brings with it from the upper regions. This must render the air inside the limits within which the rain is falling heavier than the air outside those limits, and thus co-operate, in an obvious way, with the former cause.

But in order to prove that theory is in this case borne out by observation, I adduce the two following quotations from writers of unquestionable authority.

Professor PHILLIPS, in his first Report* on the observations at York, makes the following remark:—"I have noticed in several instances the fact, that the wind which accompanies the fall of rain takes the line of the rain-drops themselves; and on the Minster, in particular, this was very strikingly illustrated when, with my friends Mr JONATHAN GRAY and Mr WILLIAM GRAY, junior, I watched the progress of a storm for thirty miles down the vale of York. The wind was insensible, except during the fall of rain, and then it came downward *with* the drops."

This testimony establishes the fact, that rain is sometimes observed to be accompanied by a downward current of air so strong as to be described as a "wind." It also records two other facts, which have a bearing on the hypothesis; the first of these is, that on the occasion on which the downward wind particularly attracted notice, the observers were not standing on the ground, but on the top of the Minster, at the height of 213 feet; and the second fact is, that the rain and downward wind began and ceased simultaneously, which affords a strong presumption in favour of their being connected as cause and effect.

The other quotation which I have to adduce is from EMERSON TENNENT'S "Ceylon,"† and is as follows:—"The first fall of rain was preceded by a down-

* Report of British Association for 1833, p. 404.

† Vol. i. p. 69.

ward blast of cold air, accompanied by hailstones, which outstript the rain in its descent."

In this case the coldness of the hailstones and the velocity of their fall produced so rapid a current as to be described as a "downward blast;" and it is probably only when the hail or rain falls with unusual violence, as in this case, that the vertical direction of the wind is perceptible to an observer at the level of the ground.

But if, for the reasons which have been adduced, it be granted that rain is always accompanied by a downward current of air, of greater or less velocity, it seems necessarily to follow that this current, originating, as it must do, in the very region of cloud, will descend charged with minute particles of water.

Having thus proved, as I hope, both by theory and observation, that rain is accompanied by a downward current of air, mingled with minute globules of water, I shall now endeavour to explain the twofold agency of such a current, in causing the indications of equal rain-gauges to vary with their height above ground.

The first and most obvious way in which the downward current produces this effect, is by filling the entire space through which the rain falls with a constantly renewed supply of very minute globules of water. The rain-drops must, of course, absorb as much of this watery medium as they come in contact with, each drop growing in size during the whole time of its descent; and the necessary result must be, that if two equal gauges receive each an equal number of rain-drops, the gauge nearest the ground will indicate the most rain.

But, with reference to the process thus described, it will naturally be asked, what becomes of the multitude of minute globules of water which are not absorbed by the rain-drops? The answer to this will, it is hoped, be found in the following explanation of the second and less obvious way in which the downward current affects the indications of rain-gauges placed at unequal heights above the ground.

Taking for granted, then, that during rain a slow current of air carrying minute globules of water is continually descending from the region of cloud, I now assume that these minute globules of water, in the course of their descent, often come into contact with one another, and coalesce into drops of sensible magnitude; and this again leads to the inference that globules which, at a higher level, descended chiefly by participating in the motion of the downward current of air, acquire, after their coalescence, a velocity and momentum which enable them not only to outstrip the current, but also to resist being carried out of their downward direction by any lateral motion which may happen to be impressed upon the current.

This process of coalescence, it is conceived, becomes more and more rapid as the resistance of the ground begins to tell on the velocity and downward direc-

tion of the current; and, judging from facts of observation, it seems to be at some point less than 40 feet from the ground that a great and sudden coalescence of the small globules of water takes place,—the effect, probably, of the current being first retarded and then forced into a lateral direction, and of the irregular mingling together of the particles of water to which this gives rise.

The hypothesis next assumes as true the following important proposition,—namely, that while a rain-gauge, placed at some height above the ground, receives all drops having a sensible magnitude, it receives none of those globules which are so minute that the velocity of their descent, though partly the effect of gravity, is chiefly due to the motion of the downward current of air on which they float. These very minute globules, it assumes, are borne by the current past the mouth of the gauge, and continue to descend until, by the coalescence of a great number of them, a drop is formed large enough to fall into a lower gauge if placed in its path.

This essential point of the hypothesis may be illustrated by what is observed when wind blows through a room having two windows opposite to each other. In such a case the leeward window is no sooner closed, than the breeze, if gentle, is scarcely felt within the room, though the other window remain open; and the obvious reason is, that the air inside the room, being now supported at all points, resists the entrance of any more air by the open window. In a similar manner, it is conceived, the slow downward current of air and floating moisture fails, as the hypothesis assumes, to find entrance into the mouth of a rain-gauge, and is forced to turn aside and to continue its descent towards the ground.

But here the question may occur, what will be the result if this downward current be combined with a current of wind at right angles to it? One effect doubtless will be, to accelerate that process of coalescence which, it has been assumed, takes place among the minute globules of water in the downward current. And this more rapid coalescence would evidently tend to increase the indication of the upper rain-gauge, and thereby to equalise it with that of the lower gauge, were it not that this tendency is more than counterbalanced by the wind uniting with the downward current of air, to turn aside from the mouth of the gauge, not only the very smallest of the globules of water, as during a calm, but also the smaller of those globules which, under ordinary circumstances, would have fallen into the gauge. It appears therefore that, as respects its ultimate effect, the wind increases the difference between the indications of the upper and lower rain-gauges, and this is in accordance with observation.

But besides the wind, which, it has been shown, increases the ordinary effect of the downward current, there are a few causes which seem to lessen or mask its influence. Thus, it is well known that, on some rare occasions, the quantity of rain in the upper gauge equals, and even exceeds, the quantity in the

lower gauge; and one or two circumstances which might tend to produce such an equality may be mentioned. If, for example, the upper gauge were placed rather close to the top surface of a tower or building, and nearer to one side of it than to the other, then, in accordance with Professor BACHE's experiments, when the wind blew from the direction of the more distant side, the indication of the gauge might be expected to be greater than if the wind blew from the near side; and thus it would not be surprising if, in certain directions of the wind, the indication of the upper gauge became nearly or quite equal to that of the lower one. Again, there are two other causes, opposite to each other in character, which, it might be expected, would tend to equalise the indications of the upper and lower gauges. The first of these is when the rain falls, more or less plentifully, but in extremely small drops; and the second is when the rain falls in large drops and very copiously. In the former case, the very small terminal velocity of the drops would give rise to a downward current of air so slow as scarcely at all to hinder the entrance of the minute globules of water into the upper gauge, or accelerate their coalescence into drops as they approached the ground; and the ultimate effect would be, a tendency to equalise the indications of the two gauges, by allowing *more* than the usual quantity of water to enter the *upper* gauge. In the latter case, again, the downward current of air might be so strong as to reach the level of the ground, and this also would tend to equalise the indications of the two gauges; but, on this occasion, it would do so by allowing *less* than the usual quantity of water to enter the *lower* gauge.

Having explained what I conceive to be the twofold agency of the downward current in producing the paradoxical results under consideration; and having also noticed some of the causes which may serve occasionally to modify these results; it would now have been desirable to test the hypothesis quantitatively; but unfortunately the want of data renders it impossible to do so in a satisfactory manner. There is, however, one point on which a numerical estimate, even if it were only a probable one, is essential. I mean respecting the quantity of water, in theshape of minute globules, which a given volume of the atmosphere must be assumed to contain, in order to account for some of the more remarkable results recorded at York or elsewhere. I shall, therefore, now endeavour to make a rough approximation to this quantity, the chief object in view being to determine whether the quantity of moisture would be so great as to render necessary the supposition of visible cloud throughout the space where rain is falling.

In selecting a case for such a purpose, recourse cannot be had to those rare instances in which 30 inches or more of rain fell in the course of twenty-four hours; for in none of these does it appear that the observations were made at more than one level.

Perhaps the winter observations at York will furnish as severe a test of the hypothesis as can be found, at least as regards the ratios of the quantities of rain

received at different levels. It is true that the maximum quantity of rain which fell *continuously* in a given time, cannot be determined by means of this series of observations, for the intervals between the consecutive observations were too long to admit of this; but still it is possible to assume a value which may be supposed, on good grounds, rather to exceed than fall short of the actual maximum rate. The following table* contains an abstract of the winter observations at York to which allusion has just been made. It exhibits the depth of rain which fell into gauges, at three different levels, during a period of 270 days, comprising the months of December, January, and February, of the years 1832–33, 1833–34, 1834–35:—

	Height of the Gauge above the ground, in feet.	Depth of Rain in inches.	Ratios of the Depths, that on the ground being 1.
Ground gauge, ...	0	17·32	1·
Museum gauge, ...	44	12·17	0·7
Minster gauge, ...	213	8·65	0·5

The remarkable fact to be learnt from this table is, that the Ground gauge received 30 per cent. more rain than the Museum gauge, the difference of level being only 44 feet; and 50 per cent. more than the Minster gauge, the difference of level being 213 feet.

As respects the rate at which the rain fell, the table shows that the Ground gauge received 17·32 inches in the course of 270 days,—that is, on an average, 0·064 inches in twenty-four hours. This, of course, is the mean rate for the whole period, including fair and rainy weather; but what is wanted is the largest quantity which fell continuously in a given time. Sir JOHN HERSCHEL states,† that "it is considered, in the greater part of England, a heavy rain if an inch fall in the course of twenty-four hours." Therefore, guided by this, it is proposed to assume, that on one of the 270 days included in the York observations it rained continuously for twenty-four hours; and that, during this period, the Ground gauge received one inch of rain, the Museum gauge 0·7 of an inch, and the Minster gauge 0·5 of an inch,—these quantities bearing to each other the same ratios which, as the fourth column of the above table shows, the whole quantities bear to each other.

Adopting, then, these data, the hypothesis assumes that, on the occasion in

* Report of Brit. Assoc. for 1835, p. 173. *N.B.*—The error in the Minster column is corrected.
† Art. *Meteorology*, par. 115.

question, and within the period of twenty-four hours, a current of very minute globules of water—equivalent, in all, to a depth of half an inch of rain—was floated downward past the level of the Minster gauge, without giving any indication of its passage; and that of this half inch, a portion, equivalent to 0·2 of an inch, changed its form while descending between the levels of the Minster and Museum gauges,—the larger part of this quantity combining to form drops of a size sufficient to admit of their falling into the lower of these two gauges, and the remainder coalescing with drops of rain already formed, and thereby rendering each drop larger than when it passed the level of the Minster gauge. The effect of this coalescence of the minute globules of water with each other, and with the larger raindrops was, according to the hypothesis, to raise the indication of the Museum gauge to 0·7 of an inch,—that is, 0·2 of an inch above the indication of the Minster gauge.

Again, it is assumed that the current, now containing a quantity of water equivalent only to a depth of 0·3 of an inch, was carried past the level of the Museum gauge without leaving any trace of its passage, and was by a similar process of coalescence, but much more rapid than what took place at the higher level, converted into drops which, owing to the cessation of the downward current as it approached the ground, were now no longer, even the smallest of them, carried past the mouth of the gauge placed there, but entering it, raised its indication to one inch,—that is, 0·3 of an inch above the indication of the Museum gauge.

Having traced the agency of the downward current thus far, the next step ought to be to ascertain the velocity of the current. But it is difficult to find any data for making such an estimate; for though it may be inferred, that the velocity of the downward current of air which accompanies rain will have some direct relation to the quantity of rain which falls in a given time, and to the degree of cold which it brings down with it, yet, unfortunately, from not knowing what that relation is in some actual instances, no use can be made of the general principle. Since, however, it is necessary to arrive at some estimate on this point, there are one or two considerations, connected with the hypothesis itself, which seem to suggest a lower limit, at least, to the velocity of the downward current.

In explanation of this, let it be assumed that (it being during winter) 3000 feet was the height of the clouds from which the one inch of rain fell in twenty-four hours. Also let it be assumed that the diameter of the drops of rain was uniformly one-tenth of an inch, which, as respects bulk, is greatly nearer the lower than the higher of the two limits to their size which Professor LESLIE has assigned. Then, one inch of rain being supposed to fall in twenty-four hours, it follows that the number of drops which would fall on any particular spot in the same time would be fifteen,* and the interval of time between the consecutive drops would be ninety-six minutes. In order, therefore, to keep the space between the clouds and the earth replenished with the minute globules of water, the

* Playfair's Geometry, Supp., B. III. Prop. xxi.

downward current ought to have a velocity of at least 3000 feet in 96 minutes,—that is, about 7 inches per second, or $\frac{1}{8}$ths of a mile per hour.

In a table given by Dr THOMAS YOUNG,* it is stated that a wind blowing at the rate of two miles an hour is just perceptible; and as the downward wind recorded by Professor PHILLIPS was distinctly felt, it may be concluded that its velocity exceeded two miles an hour. Therefore, in adopting 7 inches per second, or $\frac{1}{8}$ths of a mile per hour, as the estimated velocity of the downward current in the present case, there is, at least, the certainty that it is considerably less than a velocity which observation has proved to be possible.

And now, let it be imagined that a hollow prism reaches vertically from the level of the Minster gauge to the ground, and that the area of its base is equal to one square inch; also let attention be directed only to that portion of the current which may be supposed to descend through the prism;—it is evident that in the course of twenty-four hours, or 86,400 seconds, its volume will amount to $7 \times 86{,}400 = 604{,}800$ cubic inches. This volume consists of air and minute globules of water, the latter being, by supposition, equal in weight to half a cubic inch of water = 126·23 grains. Hence, according to this estimate, each cubic inch of the atmosphere between the levels of the Minster and Ground gauges, and within the limits of the raining space, would contain, besides the aqueous vapour due to its temperature, only $\frac{126{\cdot}23}{604800} = 0{\cdot}00021$ gr. of condensed vapour,—*i.e.*, less than half the quantity which would be requisite to saturate it, if dry, at zero of Fahr.

If, therefore, this estimate be at all near the truth, it seems to follow that even such remarkable results as those of the winter observations at York may be accounted for by the presence in the atmosphere of a quantity of condensed vapour too small to give rise to the appearance of visible cloud.

Before bringing the paper to a close, there remains to be noticed what, at first sight, might seem to be a new source of difficulty, namely, the fact that when the elevation on which a rain-gauge is placed, instead of standing detached like a house, or tower of a cathedral, forms part of a mountainous country, the ordinary effect of elevation, in appearing to diminish the quantity of rain, is no longer observed. Such a result, however, presents no real difficulty; for when the current of air carrying minute globules of water which accompanies rain descends on an elevated, but, at the same time, extended and uneven surface, the resistance offered to its downward progress ought, according to the hypothesis, to produce nearly the same effects as at the level of the ground.

In concluding this attempt to explain a long standing difficulty in the theory of rain, I do so with an unfeigned sense of the very imperfect manner in which it has been executed, but, at the same time, with a good hope that it will be found to be based on a true principle.

* Lectures, vol. ii. p. 457.

IV.—*On the Pressure Cavities in Topaz, Beryl, and Diamond, and their bearing on Geological Theories.* By Sir David Brewster, K.H., F.R.S.

(Read 3d March 1862.)

In the years 1823 and 1826 I communicated to this Society two papers "On the Existence of Two New Fluids in the Cavities of Precious Stones and other Minerals." These two fluids were generally found together in the same cavity, though sometimes the cavities were occupied only by one of them. They were perfectly transparent and immiscible. The denser of the two occupied the angles of the cavities, or the necks, or narrow passages, or canals which united two or more larger cavities; while the rarer fluid floated, as it were, on the other in deep cavities, or filled the body of shallower ones, with the exception of a circular vacuity, which diminished and disappeared with the slightest increase of temperature, or enlarged itself and disappeared in consequence of the fluid being converted into vapour.

The denser of these fluids does not appear to expand more than oil or water by the application of heat; but the other is *twenty-one* times more expansible than water. It evaporates at temperatures from 74° to 84°. The vacuity in it disappears by the heat of the mouth or of the hand, and it returns to its former state by a violent effervescence, producing a number of minute vacuities, which finally unite in one. The refractive power of the expansible fluid varies from 1·1311 to 1·2106, while that of the denser fluid is 1·2946, which is greatly less than that of water. From the few experiments which I was able to make on these fluids when taken out of the cavities, it has been inferred that they are hydro-carbons.

The distribution of these cavities, in the specimens which contain them, is a subject of peculiar interest. They are often found singly, and of different sizes, at different depths in the mineral; but they most frequently occur in strata, and of such different magnitudes, that the two fluids are distinctly seen in the largest, while the rest gradually diminish till they disappear in black points, which the microscope can hardly descry. Three or four strata nearly parallel to one another, and with cavities of different sizes, rarely occur. In general the strata lie in planes, frequently intersecting one another, and having no connection with the primitive or secondary planes of the crystal. In some specimens the planes of the strata are curved, and in rare cases the sections of these planes are curves of contrary flexure.

In 1844 I was led to re-examine several hundred specimens of topaz with a more perfect microscope and a fine polarising apparatus, with the view of ascer-

* The American and French mineralogists have given the name of *Brewstoline* to the volatile, and *Cryptoline* to the dense fluid.

taining the nature and properties of certain crystalline deposits which I had noticed, and to which I had referred in my earliest observations.* In these new researches, the results of which were published in two papers in the Transactions of this Society for 1845, I discovered two new classes of phenomena which had escaped the notice of preceding observers, and which threw much light on the formation of the minerals in which they were exhibited.

In many specimens of topaz from Brazil and New Holland, I discovered numerous cavities, filled with crystals of various primitive forms, and with different physical properties. These crystals are either fixed or moveable. Some of the fixed crystals are beautifully crystallised, and have their axes of double refraction coincident with those of the specimen which contains them. In some cavities there is only *one* crystal, in many *two*, *three*, and *four*, and in a great number the crystals actually fill the cavities to such a degree, that the circular vacuity in the fluid cannot take its natural shape, and can often be scarcely recognised among the jostling crystals.

Upon the application of heat to these crystals, some of them gradually lost their angles, and melted slowly, till not a trace of them was visible. Others melted with greater difficulty, and some resisted the most powerful heat I could apply. The crystals which melted easily were quickly reproduced,—sometimes reappearing in a more perfect form, but frequently running into amorphous shapes or granular crystallisations. While some of the crystals were resuming a tabular form, their tints, under the polarising microscope, gradually rose in the scale of colours as their thickness increased; and when there happened to be numerous crystals in the specimen, the whole field of the microscope was filled with brilliant portions of light which they polarised.

While making these observations, crystals of a different kind presented themselves to me when the specimens which contained them were exposed to polarised light. These crystals were embedded in the topaz; and as their axes of double refraction were not coincident with those of the mineral, they were seen in the obscure field of the microscope, brilliant with all the colours of polarised light. They often polarise five or six orders of colours, and in general they have beautiful crystalline forms, which are visible in the microscope even in common light. In some specimens of Brazil topaz, the embedded crystals occur in groups of singular beauty, consisting of prisms and hexagonal plates, connected apparently by filaments of opaque matter. In all these specimens the crystals had a distinct outline, whether they were examined in common or in polarised light; but I have met with topazes in which the embedded crystals had no visible outline in common light, and which never could have been detected but by the polarising microscope. In one of these, an amorphous crystal, nearly spherical, lay in a

* See Edinburgh Transactions, vol. x. p. 21, note, and plate i. fig. 10, plate ii. figs. 20, 21; p. 419, note, and plate xix. fig. 4.

crowded group of small fluid cavities, none of which had entered it,—a proof that the cavities had been formed in the topaz when soft, and when it imprisoned the previously indurated crystal.

The other class of phenomena to which I have referred is of a still more remarkable nature, and has a more direct bearing on geological theories. About thirty years ago I communicated to the Geological Society the singular fact that I had found in a diamond a small cavity, round which four luminous sectors were seen in polarised light,—a phenomenon which clearly proved that the diamond, when in a soft state, had been compressed by an elastic force proceeding from the cavity. This inference countenances the opinion that the diamond was of vegetable origin; and as this gem was a sort of outlaw in the mineral world, the idea that it had once been in a plastic state, like amber and other gums, and susceptible of compression, did not startle the mineralogists who believed in the ordinary doctrine of crystallisation. The insulated fact, therefore, and the probable inference from it, excited no notice; and it was not till the same phenomenon had been observed more frequently in the diamond, and in other minerals supposed to be of aqueous formation, that its geological importance was likely to be acknowledged.

In the Koh-i-noor diamond, which the PRINCE-CONSORT kindly permitted me to examine in 1852, I found three black specks, scarcely visible to the eye, but which the microscope showed to be irregular cavities, surrounded with sectors of polarised light. In the two smaller diamonds which accompanied the Koh-i-noor, there were also several cavities surrounded with luminous sectors, and the same polarising structure which indicated the operation of compressing and dilating forces.* In order to obtain more information on this subject, I examined nearly fifty diamonds lent me by Messrs HUNT and ROSKILL, and in almost all of them I found numbers of cavities, of the most singular forms, round which the substance of the stone had been compressed and altered in a remarkable manner. The shapes of the cavities sometimes resembled those of insects and lobsters, and the streaks and patches of colour in polarised light were of the most variegated kind. In examining a large number of diamonds, which adorn some of the oriental objects in the East India Company's Museum, I found that all these stones contained large cavities, and were coarse or flawed diamonds, which could not be cut into brilliants, or used in rings or other ornaments. It seems, indeed, to be a general truth, that there are comparatively few diamonds without cavities and flaws, and that this mineral is a fouler stone than any other used in jewellery. Some diamonds, indeed, derive their black colour entirely from the number of cavities which they contain, and which will not permit any light to pass between them.

* In 1820 I discovered similar cavities in amber, &c. See Edin. Phil. Journal, vol. ii. p. 334.

† See Edin. Trans. 1815, vol. viii. p. 157; or Journal de Physique, 1816, vol. lxxxii. p. 367.

Having found in diamond so many *Pressure Cavities*, as we may call them, round which the substance of the stone is compressed, I had some expectation of finding them in other minerals; and upon re-examining the numerous plates of topaz in my possession, I succeeded in discovering several under such remarkable circumstances, that I submitted a description and drawings of them to this Society in 1845. In searching for this phenomenon with the polarising microscope, we first observe four sectors of depolarised light; and if the magnifying power is sufficient, we shall find, in the centre of the black cross that separates the sectors, a small opaque speck, which is the cavity or seat of the compressing force. This cavity is frequently of a rhomboidal form, and often only the 3000th or 4000th of an inch in diameter. It is always opaque, as if the elastic substance which it contained had collapsed into a black powder; and I have met with only one cavity in which there was a speck of light in its centre. The polarised tint in the luminous sectors varies from the faintest *blue* to the *white* of the first order. In most cases the elastic force has spent itself in the compression of the topaz,—the cavity remaining entire, and without any apparent fissure by which a gas or a fluid could escape. I have discovered, however, other cavities, and these generally of a larger size, in which the sides have been rent by the elastic force, and fissures, from one to six in number, propagated to a small distance around them. These fissures have modified the doubly refracting structure produced by compression, but the gas or fluid which has escaped has left no solid matter on the faces of fracture.

Soon after the publication of these results, I discovered still more remarkable cavities in a specimen of beryl brought from India by the Marchioness of TWEEDDALE, who was so kind as to present it to me. In cutting the crystal, Mr SANDERSON found that one end of it was foul, and produced a luminous ring round a candle. This ring, similar to the rings seen in certain specimens of Iceland spar, was produced by long and irregularly tubular cavities, parallel to the sides of the hexagonal prism. As the tubes had been cut across by the lapidary, their contents had escaped; but whatever the contents were, whether fluid or gaseous, they had compressed the beryl, and produced the four luminous sectors around each cavity. This aggregation of luminous sectors produced a mass of depolarised light, which completely effaced the black cross of the uniaxal system of rings exhibited by the mineral. Different degrees of compression were produced by cavities of different sizes; but the resulting tint was generally a *white* of the first order, rising in some cases to a *yellow* of the same order.

Such is a brief notice of the fluid and pressure cavities which exist in minerals, and which have a very obvious bearing on geological theories. Some of these facts have been upwards of forty years before the public,* and along with

* Edinburgh Philosophical Journal, vol. ii. p. 334, 1820.

others, more recently discovered, have been widely circulated in British and foreign journals; and yet none of our geologists have made the slightest reference to them, either as difficulties to be explained, or arguments to be advanced in support of their own views.

In 1822 Sir H. Davy, when he was acquainted only with the existence in minerals of water, petroleum, and gas, did not hesitate to regard such facts as "seeming to afford a decisive argument in favour of the igneous theory of crystalline rocks;"* and in my paper of 1826, I was driven to the conclusion, "that the cavities containing the two new fluids were formed by highly elastic substances, when the mineral itself has been either in a state of fusion, or rendered soft by heat." At this time I was acquainted only with the two new fluids, and some of their chemical and physical properties; but when I had studied their arrangement in strata, this opinion acquired additional weight. Had these cavities been arranged in planes parallel to the primitive or secondary faces of the crystal, some argument might be urged in favour of their aqueous formation; but when it was found that the strata of cavities traversed the crystal in all possible directions, that they were bent also into curves of contrary flexure, and that even individual cavities had a curvilinear shape, it was impossible to resist the conclusion that the cavities were formed, and thus capriciously distributed, when the substance of the crystal was in a soft or plastic state. This conclusion derives additional strength from the fact that the water cavities in crystals deposited from an aqueous solution are never thus arranged.

The discovery of pressure cavities in topaz and diamond may be considered as completing the evidence for the igneous origin of these minerals, and of the rocks which contain them. We know that gas, in a state of compression, exists in minerals. In the pressure cavities we have not only the seat of an elastic force, but its direct action upon the substance of the crystal. Though of equal density throughout, as is proved by the equality of its polarised tints, the crystal has its density increased round the pressure cavity,—the density being a maximum close to the cavity. Such a structure is impossible in crystals formed by aqueous deposition, and hence there is not a single example of a pressure cavity in any of them. They exist, however, in amber and in glass—substances that have once been in a plastic state; and I have produced them artificially by compressing a solution of gum arabic between two plates of glass, so as to include some bubbles of air. The air in these cavities, being exposed to changes of temperature, compresses the circumjacent gum, and gives it that variation of density which produces four luminous sectors in polarised light, exactly of the same character as those which are found in topaz and diamond.

The existence of crystals of different physical properties in the cavities of minerals, and of embedded crystals either shooting through their mass, or occur-

* Philosophical Transactions, 1822, p. 367.

ring in groups, or lying singly with their optical axes in every direction, admit of no other explanation than that which is afforded by supposing the surrounding mineral to have been in a state of fusion, and to have either contained the elements of the embedded crystals, or to have surrounded them when previously formed.

Although, as I have already stated, no British geologist has seen the importance of the preceding facts, and their direct bearing on geological theories, yet they have been recently referred to,* and their value fully appreciated, by French geologists. In a discussion with M. Elie de Beaumont on the formation of mineral veins, M. Fournet,† the distinguished Professor of Geology at Lyons, has given a full and interesting account of this class of phenomena, and has adduced them to prove that mineral veins are formed by the injection of mineral matters in the state of fusion. In opposition to this argument, M. Elie de Beaumont makes the following observations:—"It is difficult," says he, "to admit that crystals of quartz containing two oily fluids, one of which is volatile at the temperature of 81° Fahr., have crystallised in a bath of quartz in fusion. But quartz forms part of the gangues of the greater number of veins, and quartz with fluid cavities is far from being a rarity."‡ M. Fournet§ has, we think, removed this difficulty; but without entering into the question as one of geology, we may safely assert that difficulties attaching to any theory are not arguments against it, especially if there are only two theories, and if equal difficulties attach to them both. We are so utterly unacquainted with the conditions under which the primitive rocks were formed, with the temperatures which prevailed at their formation, and with the pressures to which they must have been subject, that we are not entitled to charge any theory with difficulties which have their origin in our own ignorance, or in the very nature of the subject. We may never understand how the cavities in topaz have such singular and complex forms as those which I have described and delineated,—how these cavities should contain in one specimen two immiscible fluids, the one dense and the other volatile, and in another specimen various crystals, of different primitive forms and physical properties. We may never understand how a series of these cavities could have arranged themselves in lines now straight and parallel, now curved and concentric, and now radiating from a centre; or how strata of these cavities could traverse the topaz in all directions with surfaces of single or double curvature. We may not be able to explain the special difficulty started by M. Elie de Beaumont, and yet it is absolutely certain that an elastic force, emanating from a pressure cavity, could not have compressed the topaz which surrounded it, unless the mineral had been in a soft and plastic state, or in the state of fusion.

* Daubrée, "Etudes sur le Metamorphisme," 1860, p. 36.

† Comptes Rendus, &c., tom. li. p. 42, tom. liii. pp. 83, 610; and Fournet, "Geologie Lyonnaise," Lyons, 1861, pp. 533, 715.

‡ Comptes Rendus, &c., 15th July 1861, tom liii. p. 83, note.

§ Geologie Lyonnaise, p. 536.

V.—*On the Theory of Numbers.* By H. F. Talbot, Esq.

(Read 21st April 1862.)

The object of this paper will be, to give a connected view of some theorems of importance, which are often found in books rather obscurely demonstrated, and in some cases are inaccurately given, or are liable to exceptions which are not mentioned.

§ 1. *On Fermat's Theorem, and Wilson's Theorem.*

The most convenient starting-point for this investigation seems to be the well-known theorem, "If p is a prime number, and $(x+1)^p$ is expanded by the binomial theorem, all the coefficients, except the first and last, are divisible by p.

For it is obvious, in the first place, that all the coefficients are integers. If we multiply $x+1$ into itself, any number of successive times, the coefficients arise from the multiplication and addition of integers, and are therefore themselves integers.

Next, the binomial theorem gives the coefficients in the form

$$p, \frac{p(p-1)}{2}, \frac{p(p-1)(p-2)}{2\cdot 3}, \text{ \&c.}$$

Let us consider any one of these, for instance the last; then, since $\frac{p(p-1)(p-2)}{2\cdot 3}$ is an integer, the numbers 2 and 3, found in the denominator must divide some of the factors in the numerator. But they cannot divide p, it being a prime by hypothesis; consequently, they divide $(p-1)\ (p-2)$, therefore $\frac{(p-1)(p-2)}{2\cdot 3}$ is an integer. But this integer is the quotient of the coefficient divided by p. Therefore, p divides this coefficient, and so for all the others.

This is the place to introduce a convenient notation, invented, I believe, by Gauss.

If a and b are two numbers which, when divided by the number n, leave the same remainder, Gauss says that they are *congruous* to each other, according to the *modulus* n; which he expresses thus, $a \equiv b \pmod{n}$. The sign $\equiv$ is imitated from $=$ the sign of equality, and implies, not that the numbers are really equal, but that they are *equivalent* (under certain circumstances only). For, if $a \equiv b \pmod{n}$ this would not, in general be the case with a different *modulus*.

I propose in the present paper sometimes to use the word *equivalent* instead of *congruous*.

If any number a is divisible by n, it is equivalent to zero, with modulus n, which is written $a \equiv o \pmod{n}$.

To return to the last theorem.

If p is a prime,

$$(x+1)^p = (x^p+1) + \left(p \,.\, x^{p-1} + \frac{p(p-1)}{2} x^{p-2} + \&c.\right)$$

Therefore we have the congruence or equivalence,

$$(x+1)^p \equiv x^p + 1 \pmod{p}.$$

For all the other terms vanish, their coefficients being all divisible by p, whence,

$$p \equiv 0 \pmod{p} \qquad \frac{p(p-1)}{2} \equiv 0 \qquad \frac{p(p-1)(p-2)}{2 \cdot 3} \equiv 0,$$

and so on.

Take this equivalence	$(x+1)^p \equiv x^p + 1$
and suppose $x = 1$	$\therefore 2^p \equiv 1+1 \equiv 2$
Next suppose $x = 2$	$\therefore 3^p \equiv 2^p + 1$
But we found $2^p \equiv 2$	$\therefore 3^p \equiv 3$
Next suppose $x = 3$	$\therefore 4^p \equiv 3^p + 1$
But we found $3^p \equiv 3$	$\therefore 4^p \equiv 4$
And so on till we reach	$a^p \equiv a.$

a being any number. Transposing, we have $a^p - a \equiv 0 \pmod{p}$. In other words, the prime number p divides $a^p - a$, or $a\,(a^{p-1}-1)$. It therefore divides one of the two factors a, or $a^{p-1}-1$, whence we obtain FERMAT'S celebrated theorem,—"If p is a prime number, which does not divide a, it necessarily divides $a^{p-1}-1$."

Next let us consider a beautiful theorem first given by LAGRANGE. If p is any prime number, and an equation be formed of $p-1$ dimensions, whose roots are the series of natural numbers, 1, 2, 3, ... $(p-1)$, all the coefficients of this equation (except the first and last) are divisible by p.

Example.—Let the roots be 1, 2, 3, 4, 5, 6, the equation will be

$$x^6 - 21\,x^5 + 175\,x^4 - 735\,x^3 + 1624\,x^2 - 1764\,x + 720 = 0$$

and each coefficient except the first and last is divisible by 7. Assuming LAGRANGE'S theorem as proved, we can deduce a remarkable consequence from it. Let Z be the last coefficient, it is the product of all the roots, or $Z = 1, 2, 3, \ldots . (p-1)$. Z is always positive, because the equation has an even number of dimensions.

Therefore the equation may be written thus:—

$$(x_p{}^{-1} + Z) + Ax^{p-2} + Bx^{p-3} + \&c. = 0;$$

But by LAGRANGE'S theorem,

$$A \equiv 0 \pmod{p},\ B \equiv 0, \&c.$$

And therefore all the terms of the congruence may be omitted except the two first.

$$\therefore x^{p-1} + Z \equiv 0, \text{ whence } -x^{p-1} \equiv Z.$$

In other words, if Z is divided by p, it leaves the same remainder as x^{p-1} does, when divided by p, but with a contrary sign; x being *any one* of the $p-1$ numbers, which are less than p.

The simplest case is when $x=1$. In this case the theorem gives—

$$-1 \equiv Z \text{ or } Z+1 \equiv 0 \text{ (mod. } p);$$

which result, expressed in other words, is:—"If p be any prime number, the product of all the numbers less than p, or 1, 2, 3, ... $(p-1)$, augmented by unity is divisible by p."

This is the celebrated theorem, known as "WILSON's Theorem," of which neither its inventor nor WARING, who first published it, could find any demonstration. It was first demonstrated by LAGRANGE (Berlin Memoirs, 1771).

We have not employed FERMAT's theorem in demonstrating it, therefore it is well to show that the latter can be deduced from it. Thus, we have found $x^{p-1} \equiv -Z \text{ (mod. } p)$.

But we have found $Z \equiv -1$. And therefore $x^{p-1} \equiv 1$ (x being any number less than p), which is FERMAT's theorem.

§ 2. *On Associate Numbers.*

By WILSON's theorem, the product of all the numbers 1, 2, 3, ... $(p-1)$, is congruent to -1 (mod. p). Another demonstration of this is given in GAUSS's "Arithmetical Researches" (French translation, p. 57). It is there said that EULER discovered that this product, omitting the first and last numbers 1 and $p-1$, could be divided into pairs of *associate* numbers, the product of each of which is $\equiv 1$ (mod. p), while the product of the remaining two numbers, 1 and $p-1$ is obviously $\equiv -1$ (mod. p). So that the product of the whole series 1, 2, 3, ... $\overline{p-1}$, is $\equiv -1$ (mod. p), as we found before.

In the passage quoted, the following example is given:—The numbers less than 13 can be multiplied in pairs, thus:—$3 \times 9 = 27 = 1$ (if we omit the multiples of 13), which we write $3 \times 9 \equiv 1$ (mod. 13).

Also, $2 \times 7 \equiv 1$, $4 \times 10 \equiv 1$, $5 \times 8 \equiv 1$, and $6 \times 11 \equiv 1$. But, on the other hand, $1 \times 12 \equiv -1$. Therefore the whole product 1, 2, 3, ... $12 \equiv -1$.

In this theorem of EULER's, the product of each pair $\equiv 1$, with the exception of one pair, which is $\equiv -1$.

I have found that there exists another and very different system of *associate numbers*, in which the product of each pair is $\equiv -1$; and therefore, the product of the whole is $\equiv -1$ whenever the number of pairs is odd; but if it is even, in that case the product of one pair always deviates from the rule governing the rest, and is $\equiv +1$. So that in all cases the product of the whole is $\equiv -1$.

We will take the same example as before, the number 13. The *associate numbers* are 1, 12 . . . 2, 6 . . . 3, 4 . . . 7, 11 and 9, 10, the product of each pair being $\equiv -1$ (mod. 13). Thus, for example, $7 \times 11 = 77$. Rejecting 78, a multiple of 13, there remains -1. But the remaining pair of numbers, 5 and 8, produce the product 40, which, rejecting 39, a multiple of 13, is equivalent to 1. Therefore $5 \times 8 \equiv 1$ (mod. 13). It will be observed that the numbers have different associates in EULER's system and in this system, 2 being associated with 6, and not with 7, &c.; except that 1 is still associated with 12, and 5 with 8.

I will add some other examples of this new system of *associate numbers*.

If the prime number be 5, the associates are 1, 4, whose product $\equiv -1$, and 2, 3 whose product $\equiv +1$. This prime is of the form $4n+1$, therefore the numbers less than it form $2n$ pairs, an even number; therefore the product of one pair deviates from the rest, as was observed before. Other examples of this, in primes of the form $4n+1$, are, $p=13$. This case has been given before. The associates are written one over the other in the following table, and the deviating pair stands by itself:—

1	2	3	7	9	5
12	6	4	11	10	8

In the case of $p=17$ we find,—

1	2	3	5	6	7	9	4
16	8	11	10	14	12	15	13

The sum of the deviant pair is always equal to the prime number. Thus, $4+13=17$. It is worth remark, that the same holds in EULER's system, where the deviant pair are always 1 and $p-1$, whose sum $=p$.

It will make the nature of these *associate* numbers plainer, if we subtract p from each of those which exceed $\frac{p-1}{2}$. The remainders will be negative numbers, less than $\frac{p-1}{2}$. Thus, if $p=17$, writing the associates one above the other, and their product in the lowest line,

	1	2	−2	3	6	5	7	4
	−1	8	−8	−6	−3	−7	−5	−4
	−1	16	16	−18	−18	−35	−35	−16
or	−1	−1	−1	−1	−1	−1	−1	+1

Rule to find the pair of numbers which deviate from the rest. Find the number x less than $\frac{p-1}{2}$, such that $1+x^2$ is divisible by p, which can always easily be done, and has only one solution. Then x and $-x$ are the pair required.

If now we turn to primes of the form $4n+3$, the numbers less than p form $2n+1$ pairs, an odd number, $\therefore$ the product of each pair $\equiv -1$, and there are no deviations.

For example, if $p=7$, the associates are

1	2	4
6	3	5

If $p=11$, they are

1	2	3	4	6
10	5	7	8	9

We will now pass to the consideration of another system of *associate numbers*, which I do not find mentioned in the books.

Theorem.—If p is a prime number of the form $4n+1$, and the series of natural numbers 1, 2, 3, &c., be taken as far as $\frac{p-1}{2}$ (which will be of the form $2n$, and therefore an even number), then the *squares* of these $\frac{p-1}{2}$ numbers can be divided into *associate pairs*, in such a way that the sum of each pair shall be divisible by p.

Example.—Let $p=17$, $\therefore \frac{p-1}{2}=8$. The 8 squares may be divided into pairs, so that each pair is divisible by 17, as follows:—

$$1^2+4^2,\ 2^2+8^2,\ 3^2+5^2,\ 6^2+7^2.$$

It is plain that each number can have only one associate. For let a have the *associate* b $\therefore a^2+b^2 \equiv 0 \pmod{p}$. If c were another associate, we should have $a^2+c^2 \equiv 0 \pmod{p}$, and $\therefore b^2-c^2 \equiv 0 \pmod{p}$; that is, p must divide one of the factors of b^2-c^2. But these are $b+c$ and $b-c$. And $b+c$ is less than p, because b and c are each less than, or equal to, $\frac{p-1}{2}$. Much more is $b-c$ less than p.

But p cannot divide numbers less than itself, therefore a has only the associate b. It remains, however, to show, that each number has an *associate*. This follows from the well-known theorem,—"That every prime of the form $4n+1$ is the sum of 2 squares, in one way only."

Sometimes one of the squares is unity. For example, the prime 17 is the sum of $1+16=1^2+4^2$. When this happens, the other *associates* are easily deduced. Thus, multiplying the equation $1^2+4^2=17=p$ by 2^2, we have $2^2+8^2=2^2 \cdot p$, which being divisible by p, is $\equiv 0 \pmod{p}$ $\therefore 2^2+8^2 \equiv 0$, and 2 has the associate 8. Similarly, $3^2+12^2 \equiv 0$, but 12 exceeding $\frac{p-1}{2}$ or 8, we substitute for it $p-12$, or 5, $\therefore 3^2+5^2 \equiv 0$, and 3 has the *associate* 5; and so on.

But when the prime p is the sum of 2 squares, neither of which is 1, we proceed a little differently. Thus, let $p=29$, which $=4+25=2^2+5^2$. Multiply *the least* of these numbers, a, by the number which will give a product *nearest* to the prime 29, and the difference will of course be less than a. Thus, if we

multiply the congruence $2^2+5^2 \equiv 0$ by 15^2, we get $30^2+75^2 \equiv 0$, and rejecting the multiples of 29, we get $1+(-12)^2 \equiv 0$, or $1+12^2 \equiv 0$ (because $75 = 3\times 29 - 12$). And upon trial it will be found that $1+12^2$, or 145, is divisible by 29. Having thus found a pair of squares, such as $1+a^2 \equiv 0$, we find all the others from it by simple multiplication, and rejecting the multiples of 29. If we had not found this pair $1+a^2$ at first, we should at any rate have approximated to it.

Another mode is the following:—Since $2^2+5^2=29=p$ and 5 is not divisible by 2, add 29 to it $\therefore 2^2+34^2 \equiv p \equiv 0$ (mod. p), and dividing by 2^2, $1^2+17^2 \equiv 0$ $\therefore 1+(-12)^2 \equiv 0$, $\therefore 1+12^2 \equiv 0$, as before.

p being a prime of the form $4n+1$, we have in all cases $p=m^2+n^2$, and having ascertained the values of m, n, we can derive from them other numbers a, b, c, d, such that $a^2+b^2 \equiv 0$, $c^2+d^2 \equiv 0$, &c.; from which a curious theorem arises,—If the prime number p divides both a^2+b^2, and c^2+d^2, it also divides both $ac+bd$ and $bc-ad$.

Example.—29 divides 2^2+5^2 and 3^2+7^2. Therefore, it divides $5\cdot 7-2\cdot 3$ and $2\cdot 7+5\cdot 3$.

Demonstration.—Because $a^2+b^2 \equiv 0$ and $c^2+d^2 \equiv 0$, $\therefore a^2c^2+b^2c^2 \equiv 0$ and $a^2c^2+a^2d^2 \equiv 0$, $\therefore$ by subtraction $b^2c^2-a^2d^2 \equiv 0$, the factors of which being $bc+ad$ and $bc-ad$, p must divide one of them. [M.]

Permute the letters a, b, in this result, since it is immaterial which is which; therefore p divides one of the two factors, $ac+bd$, or $ac-bd$. [N.]

Comparing the results M and N, we see that if p divides $ac+bd$ in the second of them, it divides $bc-ad$ in the first.

It appears from what precedes, that a prime p of the form $4n+1$ always divides some number of the form $1+a^2$, where a is less than $\frac{p-1}{2}$. Annexed is a table of the values of a for the first prime numbers of that form, from 5 to 109.

5	divides	$1+2^2$	61	divides	$1+11^2$
13	...	$1+5^2$	73	...	$1+27^2$
17	...	$1+4^2$	89	...	$1+34^2$
29	...	$1+12^2$	97	...	$1+22^2$
37	...	$1+6^2$	101	...	$1+10^2$
41	...	$1+9^2$	109	...	$1+33^2$
53	...	$1+23^2$			

The law which governs these results is not manifest, therefore, although the prime p always divides a number of the form $1+x^2$ $\left(x \text{ less than } \frac{p-1}{2}\right)$, yet x must be found by tentative methods.

We will here add a few more examples of a theorem previously mentioned:—

The prime $13=2^2+3^2$ and divides $1+5^2$ $\therefore 3\cdot 5-1\cdot 2=13$, and $2\cdot 5+1\cdot 3=13$.

The prime $41=4^2+5^2$ and divides $1+9^2$ $\therefore 4\cdot 9+1\cdot 5=41$, and $5\cdot 9-1\cdot 4=41$.

The prime $61=5^2+6^2$ and divides $1+11^2$ $\therefore 5\cdot 11+1\cdot 6=61$, and $6\cdot 11-1\cdot 5=61$.

Although a prime of the form $4n+1$, is always the sum of 2 squares, yet a rule is wanting to determine these squares. The following answers for one case:—

Let p be the prime. Try if $2p-1$, is a square, and if so, call it g^2.

Then $$p=\left(\frac{g-1}{2}\right)^2+\left(\frac{g+1}{2}\right)^2$$

Example.—Let $p=1861 \therefore 2p-1=3721=61^2=g^2 \therefore p=30^2+31^2$.

§ 3. *Remarks on Barlow's Theory of Numbers.*

PETER BARLOW, of the Royal Military Academy, a mathematician of eminence, and author of a volume of tables most useful to all persons engaged in numerical computations, and believed to be exceedingly accurate, published in 1811 a work entitled "An Elementary Investigation of the Theory of Numbers." This book, which gives much useful information on a subject at that time little known to the English reader, contains a few errors which ought to be pointed out, lest they should acquire credit, by having appeared in a work of authority.

I. It is well known that mathematicians have never been able to find the demonstration of FERMAT'S theorem, which asserts that $a^n+b^n=c^n$, is an impossible equation, if n is an integer number greater than 2. Nevertheless, BARLOW, at p. 169 of his work, professes to give a demonstration of this theorem. Subsequent mathematicians, however, have tacitly ignored BARLOW'S demonstration, and the question has continued to be proposed from time to time by the French Institute and other learned societies, without receiving any solution. It is worth while, therefore, to inquire for what reason BARLOW'S demonstration has been put aside. Before treating of the general problem, to satisfy the equation $a^n+b^n=c^n$, he treats of the particular case $a^3+b^3=c^3$, and as he treats this exactly in the same way pp. 132–140, one explanation will suffice for all. It appears to me that the error of the demonstration lies in p. 139, where he obtains an equation $\frac{t^2}{r}-\frac{s^2}{tr}-\frac{9r^2}{st}=6$, and says, *first*, that because r, s, t, are prime to each other, each of the above fractions is in its simplest form; and, *secondly*, that they each contain a factor in their denominator, that is not common with the other denominators; and therefore, these fractions cannot, anyhow combined, be equal to an integer, by Corollary 2 of Art. 13. But this theorem is not true. Take for example the equation $\frac{7}{2\cdot3}+\frac{8}{3\cdot5}+\frac{3}{2\cdot5}=\mathrm{S}$. According to the theorem, S cannot be an integer, because the fractions are in their lowest terms, and each denominator contains a factor, *that is not common to the other denominators.*

But on trial, we find that S=2, an integer. Turning, therefore, to the Corollary mentioned, which is found at p. 20, we see that it rests upon a theorem in p. 19, viz.:—"The sum of two fractions in their lowest terms, of which the denominator of the one contains a factor not common with the other, cannot be an integer." This may be admitted; but Cor. 2, which follows, appears to be

erroneous, viz.:—" Cor. 2. *In the same manner it may be shown*, if there be several fractions, and one of them be in its lowest terms, and contain a factor in its denominator, *that is not common to all the other denominators*, the sum of these fractions cannot be an integer." As BARLOW's demonstration of FERMAT's theorem reposes on this Corollary, that demonstration falls to the ground, and a true demonstration of the theorem still remains to be sought for.

II. There is a well known and very remarkable theorem, that " Every prime number of the form $4n+1$ is the sum of two squares, and in one way only."

The most simple proof of this appears to consist in the following series of propositions:—

(1.) The product of the sum of two squares by a similar quantity is likewise the sum of two squares, and in two ways,—

Because $$(a^2+b^2)(c^2+d^2)=(ac+bd)^2+(ad-bc)^2;$$
and also, $$=(ac-bd)^2+(ad+bc)^2$$

(2.) The sum of two squares can only be divided by a quantity of like form.

(3.) By WILSON's theorem, a prime p always divides $1, 2, 3, \ldots (p-1)+1$, and this product may be written

$$1.(p-1)\ldots 2.(p-2)\ldots 3.(p-3)\ldots \&c.$$
or, $$(p-1)(2p-2^2)(3p-3^2)\ldots \&c.$$

or omitting the multiples of p, and observing that the number of factors is even, if p is of the form $4n+1$, the product may be written

$$1^2\times 2^2\times 3^2\ldots(2n)^2=[1\cdot 2\cdot 3\ldots 2n]^2=Q^2.$$

Therefore p divides Q^2+1 the sum of two squares. Therefore p is itself the sum of two squares.

BARLOW, at p. 205 of his work, gives the converse of this theorem, and says, that a number of the form $4n+1$ is necessarily a prime number, if it is the sum of two squares, in one way only. Suppose, however, that we take for example the number 45. This is the sum of two squares $36+9$, and in one way only. Nevertheless, the number 45 is not a prime, as it ought to be by this rule. This shows how much caution is necessary in writing on this branch of mathematics. The fact is, the theorem only holds good in case the two squares are *prime to each other*. Now, 36 and 9 are not so; and, consequently, the conclusion that their sum is a prime number is erroneous. With this limitation, however, I believe the theorem is correct. There is one apparent exception, however, which should be pointed out. The square of a prime number of the form $4n+1$ is of the same form, and is the sum of two squares in one way only. Thus, 5^2, or $25=16+9$ and 13^2, or $169=144+25$. The test, therefore, appears to fail in these instances. But in fact it holds good; for 25 is not only the sum of the squares $16+9$, but also of $25+0$, and this consideration applies to all similar cases.

VI.—*On the Rain-Fall in the Lake-District in* 1861, *with some Observations on the Composition of Rain-Water.* By JOHN DAVY, M.D., F.R.S., Lond. and Edin.

(Read 7th March 1862.)

Before entering on the main subject of this paper, the composition of rain-water, I shall give a brief account of the rain that has fallen in the Lake-District during the year just past; and this I shall do chiefly by means of tables. The quantity of rain registered has been so greatly in excess of any former year, that of itself it is deserving of record, and the more so, comparing the weather which has prevailed here with the weather in the south, for the most part, happily for the harvest, as remarkable for an opposite state, an excess of dryness.

The first table I shall give will convey a general idea of the meteorological phenomena during the period, as it includes in its several columns almost all the conditions appreciable, which have an influence on climate. I am indebted for it to a very accurate and zealous observer, Mr SAMUEL MARSHALL of Kendal. The plus and minus marks in it refer to averages: in the instance of the rain-gauge derived from an experience extending over forty years; in the instance of the thermometer and barometer over a period only one year less, and during the whole time using the same instruments.

The second table relates solely to the rain-fall: it contains the recorded results of the amount monthly at the several places named. For the means of forming it, I am indebted to the resident gentlemen or their agents, by whom the record has been kept. The area comprised—extending in one direction from Kendal to Keswick and its neighbourhood, and in the other direction from Coniston to Patterdale—pretty well represents the Lake-District at large.

The third table shows the number of rainy days at the several places, that is, the days in which any rain had fallen during the twenty-four hours.

The fourth table is designed to show the quantity—that an unexampled one—which fell in the month of November. As in the third table, the record of the rainfall is more limited as to localities than that of the second table, owing to the circumstance, that only some of the registrars recorded regularly their observations daily.

TABLE I.—Summary of Meteorological Observations made in Kendal in 1861.

Months.	Barometer.				Thermometer in Shade, 5 feet from the Ground.				Thermometer on Grass.		Mason's Hygrometers.		Quantity of Rain in Inches.	+ or − Mean.	No. of Rainy Days.	+ or − Mean.	Prevalent Winds.	Mean of Ozone.
	Max.	Min.	Mean.	+ or − Mean.*	Max.	Min.	Mean.	+ or − Mean.	Solar Radiation.	Terrestrial Radiation.	Dry Bulb.	Wet Bulb.						
January, .	30·279	29·290	29·957	+0·327	58°	11°	34·758°	−0·643°	38·9°	28·3°	34·1°	33·3°	3·745	−1·002	11	− 3	SW.	2·1
February, .	30·455	28·965	29·621	−0·039	51	19	39·884	+2·729	55·9	31·9	39·2	37·7	5·743	+1·302	16	+ 2	SW.	5·4
March, . .	30·155	28·626	29·453	−0·208	55	27½	42·185	+2·070	67·8	34·0	42·9	40·5	7·947	+4·030	28	+14	SW.	4·6
April, . .	30·401	29·472	29·984	+0·289	72	29	46·341	+1·438	95·5	31·7	50·8	45·8	0·547	−1·997	8	− 3	NE.	3·8
May, . .	30·219	29·454	29·902	+0·205	76	25	51·258	+0·001	110·4	35·9	56·1	50·4	0·710	−1·558	3	− 8	NE.	4·0
June, . .	30·004	29·387	29·749	+0·017	79½	40½	59·908	+2·845	103·6	44·3	65·1	59·5	3·009	−0·871	8	− 6	NE.	3·8
July, . .	29·938	29·009	29·459	−0·237	76	44	58·185	−0·910	92·9	45·0	61·9	57·9	6·886	+2·318	22	+ 7	SW.	2·7
August, . .	30·098	29·306	29·705	−0·003	76	45	59·323	+1·335	87·3	46·7	60·0	57·3	8·312	+3·348	21	+ 5	SW.	3·9
September, .	29·999	28·953	29·581	−0·104	70	38	54·575	+0·911	86·1	39·0	56·0	53·6	5·848	+1·563	17	+ 3	SW.	3·0
October, .	30·164	29·071	29·784	+0·196	64	32	51·524	+4·269	73·5	35·0	52·1	50·7	3·528	−1·947	12	− 6	SW.	3·5
November, .	30·171	28·947	29·403	−0·160	52	13½	38·700	−0·457	50·3	22·7	38·1	36·9	11·396	+5·738	23	+ 7	SW.	4·2
December, .	30·396	28·951	29·877	+0·268	53	15	37·201	−1·418	41·1	24·1	35·7	34·5	3·026	−2·494	13	− 4	SW.	2·9
Annual means, &c.	30·190	29·119	29·706	−0·046	65·2	28·3	47·820	+1·014	75·3	34·9	49·3	46·5	60·697	+8·426	179	+ 3	SW.	3·6

* The mean of the Barometrical Observations is deduced from thirty-nine years' observations; of the Thermometer from forty years.

TABLE II.

Months.	Kendal.	Lesketh How, Ambleside.	Lancrigg, Grasmere.	Wray Castle.	High Close, Grasmere.	The How, Troutbeck.	Coniston.	Seathwaite, Borrowdale.	Keswick.	Mirehouse.	Patterdale Hall.
January,	3·745	6·69	6·094	6·084	3·68	9·080	13·5	9·88	4·208	2·29	3·50
February,	5·743	12·08	13·390	8·114	9·02	13·614	9·2	18·27	9·295	7·06	14·70
March,	7·947	11·67	15·299	9·959	12·03	15·603	11·5	26·08	6·681	7·51	12·20
April,	·547	·99	1·185	·640	1·05	1·247	·3	·82	·907	·96	·60
May,	·710	1·10	1·643	1·290	1·30	·792	2·7	4·61	·550	1·41	·12
June,	3·009	4·62	3·651	3·678	3·94	2·559	4·0	7·70	3·154	2·08	1·12
July,	6·886	9·56	8·470	8·756	9·60	8·641	11·5	14·59	8·284	4·91	8·18
August,	8·312	10·96	17·503	9·527	11·61	15·913	12·0	25·20	7·580	7·02	18·25
September,	5·848	9·52	16·277	7·884	11·75	12·460	10·0	17·42	9·114	7·40	17·21
October,	3·528	5·76	9·049	4·373	8·20	5·170	5·0	9·07	3·530	3·24	7·70
November,	11·396	18·88	20·168	14·437	17·07	21·408	17·0	35·41	13·838	11·49	7·15
December,	3·026	7·20	10·490	5·166	4·50	9·771	5·5	13·62	7·270	4·98	9·10
	60·697	99·03	123·219	80·708	93·75	116·258	102·2	182·67	74·417	60·35	94·83

TABLE III.

Months.	Kendal.	Lesketh How, Ambleside.	High Close, Grasmere.	The How, Troutbeck.	Seathwaite, Borrowdale.	Keswick.	Mirehouse.†	Wray Castle.
January,	11	14	5	11	17	17	6	15
February,	16	16	10	15	17	17	12	16
March,	28	28	13	27	25	25	24	29
April,	8	7	...*	7	7	7	6	9
May,	3	8	8	6	6	6	6	10
June,	8	11	14	8	15	15	10	10
July,	22	26	24	21	23	23	22	28
August,	21	24	26	21	23	23	27	24
September,	17	19	21	17	19	19	24	23
October,	12	14	...*	11	11	11	11	17
November,	23	22	25	23	24	24	24	20
December,	13	16	15	14	17	17	16	14
	179	205	141	181	204	204	188	215

* "Not taken daily."

† Mirehouse is four miles NNW. from Keswick.

TABLE IV.

November 1861.	Lesketh How, Ambleside.	Kendal.	Seathwaite, Borrowdale.	Keswick.	The How, Troutbeck.	Wray Castle.
1	0·47	0·673	...*	0·362	0·654	0·367
2	·17	·035	...	·600	·471	...
3	...	...	...	·270	...	...
4	·10	·409	...	·184	·407	·717
5	·36	·202	2·43	·384	·817	·631
6	·98	·405	1·22	·102	·742	·101
7	·10	·057	...	·092	·123	·161
8	·11	·157	...	·210	·236	...
9	...	...	·66	...	...	...
10	·07	·088	...	·246	...	1·000
11	·85	·680	2·21	·696	1·355	1·822
12	2·73	1·588	4·24	2·150	2·305	·349
13	·17	·228	·85	·006	·360	...
14	...	·012	...	·200	·173	·097
15	...	...	·37	·220	·140	...
16	...	...	...	...	...	·118
17	·06	...	...	...	·113	...
18	...	...	...	...	...	...
19	...	·170	·17	...	...	·633
20	·79	·204	4·02	·336	·840	1·335
21	1·67	·904	4·88	·966	2·505	·895
22	1·55	·900	·45	1·012	1·510	·267
23	·31	·430	...	·082	·178	...
24	...	...	...	...	...	...
25	1·05	·393	2·38	·660	1·660	3·612
26	4·83	2·240	7·52	3·730	3·160	·062
27	·09	·065	1·00	·254	·042	·473
28	·37	·440	...	·036	·492	·903
29	1·30	·689	...	·300	1·945	·470
30	·75	·427	3·01	·740	1·180	·428
	18·88	11·396	35·31	13·838	21·408	14·441

The general results, as shown in these tables, may be pointed out as briefly the following :—

1. A prevalency of south-west winds.
2. The annual temperature very slightly in excess of the mean.
3. The atmospheric pressure very slightly below the mean.
4. The number of rainy days slightly in excess, and varying at the different stations, and in no regular ratio with the quantity of rain.

* The gauge at Seathwaite, I have been informed, was not examined daily, only on the days specified in the table. As observations were made on the 25th, 26th, and 27th, there can be no doubt respecting the correctness of the amount, 7·52 inches, during the twenty-four hours.

In the preceding table, the number of rainy days at Seathwaite is given as the same as at Keswick, where a strict account was kept. It is believed by those acquainted with the two localities, nine miles only apart, that when there is rain at Keswick there is a certainty of rain at Seathwaite.

5. The total amount of rain greatly in excess; the excess, with few exceptions, increasing towards the higher mountains of the district, and especially at Seathwaite in Borrowdale, a spot nearly central amongst them.

This excess of rain at all the stations, I may remark, has not only been considerable, but even unprecedented. Mr John Dixon, the observer at Seathwaite, states, that there the quantity registered has "exceeded any former year, for the last fifteen years, by 22 inches,"—this period comprising, I believe, the whole time that a record of the rain fallen has been kept there: and from Mr W. Rumney, in the employ of James Garth Marshall, Esq., I learn that the rain-fall this year at Coniston is as much as 23 inches in excess of the average of the last twenty-five years.

I need hardly remark that such an excess of rain, especially the fall in November, was productive of floods. These, though they occasioned some damage, have been less destructive than might have been expected, probably owing to the peculiar features of the country, the many receptacles for water, such as the lakes afford, and the ready discharge of water, where there are no lakes, through well worn and deep water-courses: moreover, the agricultural character of the district—so much of it pastoral, so little of it broken up by the plough—may have been another safeguard.

A rainy, cool season, is commonly a healthy one. This year has not, I believe, been an exception. The health of the inhabitants generally, from all I can learn, has been equal to, if not above the average. I have not heard of any unusual sickness during the months of most rain, except at Keswick and at Bowness, at each of which I am informed typhus or typhoid fever has prevailed, and has been in several instances fatal. The occurrence of the disease in both has been attributed, and probably justly, to neglected drainage, and to the neglect of other sanitary measures, in Keswick especially, in the lower part of the town, which is subject to being flooded. I shall now proceed to my main subject.

The observations which I have made on the composition of rain-water were begun in August 1860, and, with occasional interruptions when from home, have been continued up to the present time—and this daily, whenever there has been any rain.

Two methods have been employed in conducting them, one microscopical, the other chemical.

First, Of the Microscopical.—These observations have been made either on single drops of rain, or on two or three collected on a glass-slide, and evaporated to dryness at a low temperature, and, as soon as dry, subjected to the microscope, using an object-glass either of one-eighth, or of a quarter of an inch focal distance. Oftener, however, a larger quantity of rain-water has been employed, taken from the rain-gauge, viz., one or two measures, the measure holding twenty-five grains of water, which has been evaporated in a watch-glass, and

when reduced to a drop or two, has been poured on a glass-slide, and reduced to dryness in the same manner as the first mentioned.

Secondly, Of the Chemical.—The rain used in these trials has always been collected in the rain-gauge. The ordinary test employed has been a solution of nitrate of silver, the extreme delicacy of which is so well known. And, besides its great delicacy, it has the advantage of taking effect immediately. It has commonly been used without any concentration of the rain-water by evaporation. Occasionally the whole, or the greater portion of the rain collected in the gauge during the twenty-four hours, has been reduced by evaporation to a small volume, and in this state has been made the subject of experiment.

The rain-gauge, it may be mentioned, is of copper, both the funnel and the vessel the recipient. It stands about $2\frac{1}{2}$ feet from the ground on a grass-plat in the garden adjoining my house, which is about a quarter of a mile in a northerly direction from the village of Ambleside, and is about 140 feet above the sea-level, and about 30 feet above Windermere.

Having premised thus much, I shall now give some of the results, and in the order just sketched out.

1*st, Of the Rain-drops.*—I shall describe from my notes, taken at the instant the observations were made, a certain number of these results, specifying the kind of weather which prevailed at the time. They are at least recommended by their simplicity, and from being free as much as possible from any source of error. I shall begin with one which was obtained before I entered regularly on the inquiry.

1. *June* 2, 1858.—The sky, at 1 P.M., after a fine early morning, became unusually overcast with dark clouds, producing an obscurity exceeding that occasioned by the then last eclipse, that of July 1851. Some rain fell in large scattered drops. A few were collected on a glass-slide. They left, when evaporated, a circular stain, distinguishable by its greyish hue, most strongly marked in its marginal outline. Under the microscope it was seen to consist of dark particles chiefly; they were of irregular forms, like soot particles, and intermixed with them there were some minute crystalline groups.

2. On the 30th August 1860, after a fall of ·90 inch of rain during the preceding twenty-four hours, the wind westerly and strong, there were occasional showers in large drops. A few of these drops, evaporated on a slide, exhibited under the microscope a delicate crystallization. Drops from another shower during the day showed the like crystallization, with which was one crystal, a cube, like that of common salt, and about $\frac{1}{80}$th of an inch in diameter.

3. *September the* 1*st.*—Showery; the wind from the same quarter; the raindrops smaller than those of yesterday. Some of them leave no stain on glass; some only a just perceptible stain.

4. *September the* 6*th.*—During the night a slight shower (·01 inch). Rain-

drops collected in the usual manner, left a circular marginal stain, conspicuous to the unaided eye. Under the microscope it was seen to be formed of minute crystals, of numerous granules, and of many spheroidal particles like pollen.

5. *September the* 8*th*.—Slight rain in the afternoon, the morning overcast; ·06 inch of rain only during the preceding twenty-four hours. A few drops, collected just after the rain began, left only a just perceptible stain, in which, under the microscope, there was no appearance of crystals, but merely of fine particles (dust particles?), and other particles, from their form conjectured to be of pollen.

6. *September the* 10*th*.—This morning there was much dew on the grass. A few drops of it collected on a glass-slide, showed no crystals, only many particles like those of pollen.

7. *September the* 14*th*.—The wind S.W. and strong; the sky overcast with dark drifting clouds; showers frequently. The rain-drops left a well-marked stain, and under the microscope a marginal crystallization was seen very like that of muriate of ammonia. An hour later the drops collected yielded a similar stain, but less strongly marked. Later still in the day, the drops collected—these larger than the preceding—left a strong stain, in which were seen some cubical crystals like those of common salt, two or three spearhead-shaped, and others, which were more like muriate of ammonia crystals.

9. *September the* 22*d*.—Yesterday fine, the wind northerly and gentle; ·33 inch of rain during the night. Some drops of rain then collected left a faint marginal stain, in which were a few minute crystals.

10. *October the* 5*th*.—A drop of rain from a brief shower left a distinct stain in which were many crystalline forms, some cubic, some dagger-like, some arborescent.

11. *October the* 16*th*.—During the preceding twenty-four hours 2·07 inches of rain; the wind still westerly; showers now (10 A.M.) of heavy drops. Some drops collected left no stain. Five hours later, viz. at 3 P.M., the result was different; drops equally large left a well-marked stain, in which were seen stellate and dagger-like crystals.

12. *January the* 26*th*, 1861.—During the twenty-four hours ·20 inch of rain; the wind southerly, and unusually strong. A rain-drop left a distinct stain, in which were some cubical and stellate crystals.

13. *June the* 23*d*.—During the night ·01 inch of rain, the wind N.E. and gentle; this morning a slight shower of heavy drops. One collected left a distinct stain, in which were visible two or three stellate crystals, one cubical, bluish, and many granules.

14. *July the* 2*d*.—During the last twenty-four hours ·19 inch of rain; cloudy, showery, the wind N.W. A drop of rain collected left a marginal stain, chiefly granular.

15. *November the 7th.*—During the last twenty-four hours ·05 inch of rain; the wind northerly; sky overcast; showers. A rain-drop left a just perceptible marginal stain, which was of a brownish tinge, and was darkened by exposure to a charring heat.

These results may suffice to show the use of the microscope in this inquiry. I shall defer any remarks on them for the concluding parts of my paper.

Of the results obtained by the use of the nitrate of silver, which was so constantly employed, I must limit myself to a brief account; and, as in a large number of instances a portion of the water tested—a measured portion, such as that specified—was evaporated for microscopical examination, it will be proper to conjoin a notice of the results thus obtained.

The following is a classification of the results, according to the degree of effects, that is, of cloudiness and precipitation, by means of the nitrate of silver. The trials made were 217 in number.

1. In 10, there was no visible effect.
2. In 29, the effect was just visible.
3. In 70, it was slight, and very slight.
4. In 57, it was moderate.
5. In 43, it was considerable.
6. In 8, it was strongly marked.

The circumstances as to wind and weather under which these results were obtained, were of course extremely various, nor were they always well defined in relation to the effect. I shall offer a few brief comments on each class.

1. The ten instances in which no effect was visible, the rain-water remaining perfectly clear after the addition of the solution of nitrate of silver, afford a striking example of the remark just made. In the larger number the atmosphere was calm, the quantity of rain small; in the smaller number there was a considerable fall of rain; the wind was gentle or moderate, and from the N.W.; in one instance it was strong and from the south, and accompanied with much rain. Though no chlorine or other substance was present appreciable as a precipitate by nitrate of silver, yet in one or two trials there was a slight discolcration of the water produced after exposure to light, indicative of the presence of organised matter. And in two or three instances that a portion of the water was evaporated, a residue or faint stain was obtained, which in one, as seen under the microscope, consisted of extremely minute granules; in the other two, of granules and of delicate stellate crystallisations.

2. In the twenty-nine instances in which the effect was only just visible, the circumstances of weather were even more various than in the last. Most frequently the wind was from the N., N.E., or N.W., and oftener gentle than strong, and with little rain. The water, when evaporated, yielded commonly a stain or residue very similar to the preceding.

3. In the seventy instances of the slight, and very slight effect, the wind was nearly as often from the south as from the north, and from the S.W. as from the N.W., and in point of strength was more frequently gentle than strong, or even moderate. The portions of water evaporated afforded a stain somewhat more strongly marked than the preceding, and, on the whole, more frequently and distinctly crystalline.

4. In the fifty-seven instances of moderate effects, the prevailing winds were S.W. and S., and most commonly gentle; a few times the atmosphere was calm and misty. The water evaporated left a stronger stain than the preceding, and with more variety of crystalline forms, of which the cube, like that of common salt, was mostly one, and often a crystallization resembling that of muriate of ammonia.

5. In the forty-three instances in which the effect was considerable, the wind was almost invariably from the W. and S.W., and either strong, or blowing in gusts, in squalls, and was accompanied by all degrees of rain. The water evaporated yielded a still stronger stain or residue than the last, but of nearly similar composition, judging from the crystalline forms.

6. In the smallest number of instances, the last class, in which the effect was most strongly marked, the wind was chiefly from the W. and S.W., seldom from the N. or N.E., and was almost constantly very strong. The residue obtained by the evaporation of a portion of the rain-water was most considerable of all in quantity, and in variety and distinctness of the crystallization.

I may remark, generally, that in all the instances in which the residue was well marked, there was a partial deliquescence of the crystals when exposed over night to a damp air, those remaining being commonly minute prisms or plates; moreover, that when a drop of muriatic acid was added, the appearance of the crystallization indicative of muriate of ammonia was increased; and further, that when the slide of glass holding the residue was exposed to a charring heat, there was almost constantly a darkening effect, demonstrative of the presence of organised matter. I may add, that there was no well-marked difference in the results, so far as I could judge, when hail or snow were thawed and subjected to the same trials, the effects of the nitrate of silver seeming to be influenced more by the direction of the wind, and its strength, than by any other appreciable circumstance. As regards the seasons of the year, the effects appeared to be generally most distinct during the winter months, when gales were most prevalent, and the wind accompanying the rain was chiefly from the W. and S.W.

I have next to state the results which were obtained by evaporating and testing, when reduced to a small volume, comparatively large portions of rain-water, each the amount of rain-fall collected in the gauge during twenty-four hours.

The first portion that was examined was the greater part of that which was found in the gauge in the morning of the 26th November, and which had fallen

during part of the preceding day and night—that is, in somewhat less than twenty-four hours. The wind was mostly S.W. and strong, and that there was a gale at sea was denoted by the presence of gulls in the valley. The temperature of the rain-water, as poured from its receptacle, was 48°. The effect of nitrate of silver on it, employed in the usual manner, was moderate. Two measures—that is 50 grs.—evaporated, left a well-marked stain, in which, under the microscope, were to be seen a few cubic crystals, a slight arborescent crystallization, and many granules. Of the whole 4·83 inches, a portion equal to ·23 inch was used in these preliminary trials. The remainder, viz., 4·60 inches, were divided into two portions,—a larger, 3·30 inches, equal to about 5·78 pints; and a smaller, 1·30 inch, equal to about 2·46 pints. The first was evaporated without any addition. To the second, before evaporation, a drop of muriatic acid was added.

The first was reduced to 356 grs. This was found to be of specific gravity 10·017. A part of it was expended in test-trials, the results of which were the following:—A slight acid reaction, and an indication, using appropriate tests, of the presence of muriatic and sulphuric acid, of soda, lime, and magnesia, with a trace of alumina, phosphoric acid, and copper. The second was reduced to 200 grs. Evaporated to dryness in a weighed capsule, it yielded a residue of ·45 grs., of a light brown hue. Heated in a glass tube it became blackened, charred, and a fine sublimate was condensed in the upper part of the tube; this, seen under the microscope, had a delicate crystalline appearance, like that of muriate of ammonia, and had no alkaline reaction, until a few drops of liquor potassæ were added, when there was a decided production of ammonia, a fume appearing on the approach of muriatic acid properly diluted, and a slip of litmus paper reddened, recovering its blue colour when put into the tube.

The smaller portion of the rain-water, that which slightly exceeded two pints, to which the drop of muriatic acid had been added, was evaporated until reduced to 94 grs. A drop or two of this, evaporated to dryness on a glass slide, left a residue in which cubical crystals, like those of common salt, were most numerous, intermixed with which were a few prisms like those of phosphate of lime, a few tabular ones resembling those of sulphate of lime, and others like muriate of ammonia in its crystallised state, and there were stellate and dagger-like crystals amongst them. This smaller portion, so reduced, subjected to the same tests and trials as the larger, afforded similar results, with the exception, that there appeared to be more muriate of ammonia present, leading, of course, to the conclusion, that the rain-water contained a little carbonate as well as muriate of ammonia. After exposure to moist air, all the crystals described deliquesced, with the exception of the plates and prisms, and those of a stellate form.

Several other specimens of rain-water, these varying in volume from one to two and three pints, have been evaporated, until, like the preceding, they were reduced to a small quantity and then tested. The results were chiefly the fol-

lowing:—When nitrate of silver, in the first instance, produced a distinct effect, then the results were very similar to those last described, showing a predominance of common salt and of magnesian salt. When, on the contrary, nitrate of silver had little or no effect on the unconcentrated water, then the results were marked chiefly either by the entire absence of common salt, or by an only just appreciable presence of it, and by the presence of lime and ammoniacal salts. In most instances the evaporated water showed an acid reaction, and afforded proof of the presence of sulphuric acid; and in most there was a trace of copper, derived, it must be inferred, from the gauge, and owing probably to the action of the acid existing in the rain.

What are the inferences that may be drawn from these observations and experiments?

1*st*, Do they not seem to accord in this, that rain-water, such as falls in this part of England, is rarely, if ever, perfectly pure, but contains something almost constantly—indeed, may it not be said constantly—derived either from the sea or land, from town or country?

2*d*, That when the wind is from the sea, and especially when it is strong, the ingredients most predominant in the rain are those which exist in sea-water, the same that washes our shores?*

3*d*, That during a calm state of the atmosphere, or with a gentle wind, and from the N. or N.E., rain-water is least impregnated with foreign matter—least with those likely to be derived from the sea—more with those likely to be derived from the land, and especially from our great manufacturing towns, such as ammoniacal salts, salts of lime, organised matter, and soot the matter of smoke, itself very compounded?

If these inferences are borne out by the results, are they not in accordance with the nature of the atmosphere, considered as the great receptacle of all matters volatile, and of all such as are capable, when in a very finely divided state, of being mechanically diffused by the action of the winds?

Facts in proof of such diffusion of matter by the winds are so well known as not to require being insisted on. I shall briefly advert only to a very few. After severe gales, common salt has often been found deposited in a quantity so considerable, as to attract the attention of ordinary observers, and this more than fifty miles from the nearest sea, even in parts of our midland counties most remote from our shores.† Volcanic dust, and dust from the deserts of

* Sea-water which I have examined, taken from the neighbouring sea, I have found to contain, besides the ordinary ingredients of sea-water, lime in a notable quantity, some alumina and sulphuric acid, and traces of ammonia and phosphoric acid.

† I have been informed by a friend residing at Meltham Parsonage, and by another residing at Armitage Bridge, both places in the neighbourhood of Huddersfield, and about eighty miles from Scarborough, and about sixty from Liverpool, the nearest ports of the opposite coasts, that after the great storm of January 1, 1839, salt was observed deposited on the leaves of the trees at both places;—they were about four miles apart.

Africa,* have fallen over vast tracts of country, at almost incredible distances from their source, and have occasionally been conveyed by upper currents of air, blowing in a direction opposite to that of the prevailing winds.† The smoke which is poured out in such large quantities from the innumerable chimneys in our great manufacturing towns is of wide range, often reaching the Lake-District, and indeed it may be said normally, so as to occasion the discoloration of the fleeces of the flocks on the higher fells, and not unfrequently to appear as a film on the surface of our lakes and tarns.‡

In relation to the economy of nature, this diffusion of matter by the agency of the winds and by means of rain, cannot but be of importance; nor, is it, I apprehend, without interest as concerns the works of man. It seems, as it were, to establish a circulation of matter, associated with that of the circulation of water, thereby bringing back in part to the land what has been abstracted from it by water, in its descent to the sea; and thus tending to prevent the exhaustion of soils which are not artificially manured, and which, in wild pastoral districts, such as our downs and fells, are constantly being depastured. It may help to account too, not only for the continued growth of herbage in the situations just mentioned, during an indefinite term of years, but also for the growth of trees and shrubs unaided by manure, and which is so rapid in the Lake District, where the rain-fall is so great.

As regards the works of man, the influences of rain, referrible to its contents, seem to be all rather of an unfavourable and destructive, than of a beneficial and preservative and restorative kind; as tending to promote the disintegration of stone employed in building, of which there are so many striking examples; and also the decay of wood, and of all tissues, whether animal or vegetable, used in the arts, to which rain has access.§ On metals too, on all such as are liable to

* On the 15th May 1830, there was a remarkable shower of dust, supposed to have come from the African Coast, which fell over a vast extent of the Mediterranean; it was witnessed about the same time of the day at Malta, Sicily, and Sardinia. An account of it is given in my "Notes on the Ionian Islands and Malta," vol. i. p. 300.

† An instance of this kind occurred during the last eruption of the Souffrière Volcano, in St Vincent, on the 30th April 1812. Although Barbadoes is sixty miles to windward, volcanic dust on that day, carried by an upper current, fell over the whole extent of the island, and was so dense in falling, as to produce the darkness of night at mid-day, and was in so large a quantity, that I saw remains of it in the soil, where it had not been disturbed, when I was there, thirty-six years after the event.

‡ In No. 73 of the "New Edinburgh Philosophical Journal," there is a notice which I communicated, relative to this sooty deposit; and in the same paper, I mention, that after Baron Liebig, I found ammonia in rain-water, but never, as he did in Germany, obtained any traces of fœcal matter.

§ If a wall for example, one such as I have watched, is not strongly impervious to drifting rain, that portion of rain which penetrates and reaches the inner surface will there evaporate and leave its saline contents,—these always accumulating, and, from their tendency to deliquesce, from attracting moisture, must render the surface almost perpetually damp,—a state equally favourable to the decay of paper-lining, and to mildew-growth.

In the West Indies, at Barbadoes, I have witnessed a like accumulation of saline matter derived

oxidate, rain, especially when acid, which it so commonly is, cannot but have effect, and that of a destructive kind, by promoting rust.

In this admixture of the beneficial and the injurious, of the restorative and destructive, we have only another exemplification of Nature's agencies, the balance in them generally, as in these specially, turning, we believe, in favour of the good.

In conclusion, may I venture to express the hope, that this inquiry will be further prosecuted, and in many different and distant stations, near to and remote from the sea, hardly doubting that results will be obtained which, whatever they may be, can scarce fail to be instructive, and which possibly may throw light on effects, especially of the nature of blights and miasmata, of which our knowledge at present is chiefly confined to the diseases which they produce.

from the air, even where no rain could penetrate; for instance, on mosquito-curtains, in an inner bedroom, merely well ventilated. In my work "On the West Indies, before and after Slave Emancipation," I have pointed out the tendency to decay from climatic agencies as one of the causes of the neglect of the fine arts in those colonies.

Lesketh How, near Ambleside,
February 8th, 1862.

VII.—*On the Structure of the Chondracanthus Lophii, with Observations on its Larval Form.*

By Wm. Turner, M.B. (Lond.), F.R.S.E., and H. S. Wilson, M.D., Demonstrators of Anatomy in the University of Edinburgh. Plate III.

(Read 20th January 1862).

The very curious Parasitic Crustacean, the structure and larval form of which we propose to consider in the following communication, we obtained in the month of August last, in considerable numbers, in the branchial pouches of a *Lophius piscatorius*, which we captured at Whitby in Yorkshire.

The attention of naturalists appears to have been first directed to this species of Chondracanthus by Dr George Johnston, of Berwick, in a short paper, illustrated by drawings of its external form, which was published in Loudin's Magazine of Natural History.* Very shortly afterwards the Danish naturalist Kröyer† described the animal, and evidently being unaware of Johnston's brief notice, applied to it the name of *Chondracanthus gibbosus*. Cuvier‡ had, however, in the Règne Animal, 1829, figured, though somewhat roughly, under the genus Chondracanthus, an animal bearing a considerable resemblance to the form now under consideration, but he did not apply to it any specific name. Rathke,§ in 1840, published the most complete account of the structure of the animal that had up to that time been recorded. His description comprised the internal and external anatomy of the female, and partially of the male. In the same year Milne Edwards‖ placed, in his classification of the crustacea, the species *Chondracanthus gibbosus* of Kröyer, along with the *Chondracanthus Delarochiana*, a species found by De la Roche on the Tunny, and with these he classed the *Lernentoma Dufresnii* of De Blainville. In 1847 Mr W. Thompson¶ stated that he had obtained this parasite from specimens of the Lophius caught in Dublin and Belfast Bays in 1839 and 1841. Van Beneden,** in 1851, gave a brief

* 1836. Vol. ix. p. 81. Paper dated December 23d, 1835.
† Natur. Tidskrift, p. 252, bd. i. Hef. iii. 1836. Oken's Isis, 1840, p. 738.
‡ Vol. v. pl. 15, fig. 3.
§ Beiträge sur Fauna Norwegens. Nov Act., vol. xx., 1843.
‖ Hist. Nat. des Crustacés, vol. iii. p. 499.
¶ Annals of Nat. Hist., vol. xx. p. 248.
** Recherches sur quelques Crustacés Inferieurs. Ann. des Sciences Nat., 1851, p. 104.

description of the parasite. The most lengthened account of the animal, which comprises a description of the male and female, and for the first time, of the larval form, has been recently given by CLAUS.*

As there are various discrepancies of statement in the descriptions of the above authors, and as we have been able more fully to elucidate several points in the anatomy of the animal, we have been induced to place an account of it before the Society. Owing to the diverse forms presented by the female, the male, and the larva, it will be necessary to consider them under separate heads.

Female.—Average length of body $\frac{2}{3}$ths to $\frac{1}{2}$ inch, greatest breadth, $\frac{1}{3}$th of inch. Body elongated, capable of division into a cephalic, thoracic and abdominal part. Ventral surface concave. Dorsal surface convex, especially when the animal is irritated during life, when it throws its back into a highly arched form.

Cephalic part about $\frac{1}{4}$th the size of thoracic. Length and breadth nearly equal. Like the other divisions of the body, it has a perfect bilaterality. On each lateral half of the dorsal surface of the head is a chitinic plate, which, reaching inwards to the dorsal mesial line, forms, by apposition of its margin to that of its fellow, a well-marked chitinic rod, which extends as far as the anterior margin of the head. From each side of the head a short process projects backwards and outwards.

To the anterior margin of the head two well-marked processes, or antennæ, are connected (fig, 1, 2, 5, a). Each arises immediately on one side of the middle line by a constricted peduncle, beyond which the antenna expands into a much broader portion, flat on the dorsal, convex on the ventral aspect, which passes outwards, and then, diminishing considerably in size, bends slightly backwards, and terminates in a bluntly conical extremity. The antenna presents, towards its free end, two slightly marked constrictions, which would appear to indicate its division into three segments. Springing from the two distal segments are several stiff conical hairs, the central part of each of which may be seen, through the transparent chitinic covering, to be continuous with the parenchymatous substance of the antenna (fig. 3). It is possible that these papilla-like hairs may serve the purpose of touch organs. Situated immediately behind the antennæ, and arising quite from the ventral aspect of the head, are a pair of hook-like processes, which are important, because it is through them that the parasite attaches itself to the branchial membrane of the fish which it infests. Each hook possesses a very well defined, highly curved form, the convexity being directed outwards, the point inwards (fig. 4, A, B). In shape it represents a three-sided pyramid, with the angles rounded. It is

* Ueber der Bau und die Entwickelung Parasitischer Crustaceen, Cassel 1858.

Synonyms:—*Chondracanthus Lophii*, JOHNSTON, RATHKE. *Chondracanthus gibbosus*, KRÖYER, W. THOMPSON, VAN BENEDEN, CLAUS. *Lernentoma Lophii*, BAIRD.

divided by a slight constriction into two parts—a terminal hook and a basal shorter portion. It is attached to the ventral aspect of the animal by a very peculiar arrangement, which is worthy of special notice. On each side of the middle line of the ventral aspect at this spot is a triangular stage or frame, the sides of which are formed of bands of chitin, and the inner angles of which meet in the mesial line. To the whole of the outer, and the adjacent parts of the anterior and posterior sides of this frame, the basal segment of the great hook is closely connected; but to the remaining portions of these sides it is attached by a loose piece of integument, so that, when the hook moves inwards to the mesial line, *i. e.*, when the inner angle of its basal segment is depressed, the integument at this spot has a plicated appearance; on the other hand, when the hook moves outwards, it is put on the stretch. The movements of the hook are regulated by transversely striped muscles: the one attached to the internal angle of the basal segment—the flexor muscle—being more strongly developed than the external, or extensor, muscle. The outer coat of the hook is hard, and even brittle, from the quantity of chitin substance which enters into its composition.

Situated behind the hooks, and at a short interval from them, are three pairs of well-marked foot-jaws (fig. 5, c). Immediately in front of the bases of the first pair of jaws, and consequently placed between them and the hooks, is a transversely oval opening, the mouth (fig. 5, s). It appears to a great extent to be overlapped by a fold of skin, which passes backwards from the framework of the hooks. When the hooks are approximated to the jaws, this fold of skin completely conceals the oral aperture. Rathke states that the mouth is situated upon a small snout-like elevation. In our various specimens we were unable to see this proboscis, which may probably be accounted for by the fact, that when we proceeded to examine into the anatomy of this part of the animal, it had been for some time in spirit, the snout perhaps only being projected at the time when the animal is sucking. Van Beneden and Claus, again, appear to think that the mouth is surrounded by the jaws; but repeated observations have convinced us that this opening is situated in front of the first pair, though perhaps slightly between their bases.

The three pairs of jaws are situated a little to the outer side of the middle line, one behind the other, in regular order. The first and second pair are so close together that the first is partially overlapped by the second. The third, on the other hand, lies at a greater distance from the other two, and somewhat further removed from the middle line. Each of the first pair (Mandibles, Rathke) consists of two segments, a basal, short and thick, and a terminal, on which the cutting apparatus is placed (fig. 6, c). The terminal part is formed of two chitinic plates, which are blended together by their anterior margins; the posterior margins are separated by a longitudinal furrow, and

fringed with a number of short, closely-set teeth. The plates gradually come to a point at their free extremity. The basal segment has a well-marked tubercle, bearing two or more conical papillæ projecting from it, close to its junction with the terminal segment.

Each of the second pair of jaws (Maxillæ, RATHKE) also consists of two segments (fig. 6, D). The terminal is formed of but a single thick plate, which, very slightly curved, comes to a fine point at its free end, and bears upon its posterior margin a row of closely-set pointed teeth, somewhat longer and more pointed than those of the first pair. The basal segment is longer than that of the first pair, and supports an elongated body, pointed like a style, which, like the terminal part of the jaw, projects towards the ventral mesial line. A short stunted papilla, corresponding in its character to those of the first pair of jaws and of the antennæ, projects from the base of the style.

Each of the third pair of jaws (Kieferbeinen, RATHKE), much longer than the first and second, consists of three segments (fig. 6, E). The terminal segment is formed of a chitinic plate, pointed at its free end, and totally devoid of teeth. It articulates with the middle segment, which is partially embraced by a peculiarly folded chitinic plate, having on its free margins a row of bristle-like projections. It would appear as if the middle segment, by the contraction of the muscular apparatus connected to it, could, together with the base of the terminal segment, be drawn between the laminæ of the folded plate. The basal is the largest of the three segments. It contains two muscles, a flexor and an extensor, for the movement of the second segment upon it.

The cephalic is separated from the thoracic part by a constricted portion, which, for descriptive purposes, we may term the neck (fig. 1, t), but which, morphologically, is probably to be regarded as the first thoracic segment, for the first pair of limbs is connected to its under surface. The thoracic part constitutes by far the largest division of the animal. Projecting from its dorsal and ventral surfaces, and from its sides, are a number of processes, which give to the animal its characteristic appearance. The lateral processes form lappet-like prolongations. On each side they are five in number, and consist of two pairs and a terminal single process. The two processes in each pair consist of a larger anterior lappet extending downwards and backwards, and of a smaller posterior, upwards and backwards. The terminal lateral process passes also downwards and backwards, and, with its fellow on the opposite side, gives to the animal the peculiar forked appearance which distinguishes it posteriorly. The sides of the thorax, immediately behind the origin of the smaller posterior lappet of each pair, present two slight constrictions, which would appear to indicate its subdivision into three smaller segments; if to these we add the neck, four thoracic segments are indicated. The dorsal mesial processes we find to be seven in number; and in this particular we agree with the earlier observers, JOHNSTON

and KRÖYER. RATHKE and VAN BENEDEN have described only five; but it is probable that they had not examined fully formed individuals. Of these processes four are much larger than the other three. Of the four large processes, the first springs from the anterior margin of the second thoracic segment (the neck being regarded as the first), the next two from the constrictions indicating the commencement of the third and fourth segments, and the fourth from the extremity of the fourth segment, from its attachment to the termination of which it presents the appearance of a tail. The three smaller processes arise intermediate to, and are overlapped by, the larger processes when the animal is seen from the dorsal aspect. The ventral mesial processes are only two in number, and correspond in their position on this aspect to that of the second and third larger processes on the dorsal aspect. Attached also to the ventral surface are two pairs of feet (figs. 1, 2, dd). The first of these springs from the under surface of the neck close to its junction with the head. It is situated a short distance behind the last pair of foot-jaws. Its outer margin extends obliquely forwards towards the cephalic lateral process, whilst the inner, much shorter, is continuous with the corresponding margin of the opposite foot. The second pair arises farther back from the posterior part of the second thoracic segment. Like the first pair, their origin is oblique, so that the outer margin extends as far as the root of the great lateral process of this segment, the inner being in close relation to the first ventral process. Each foot is unequally bifid at its extremity, and presents, in addition, four small tubercular elevations.

The abdominal portion (fig. 2, g, fig. 8), very small, is situated between the last lateral thoracic lappets, and is overlapped by the last dorsal mesial process. It is distinctly divided into two segments, the proximal of which has connected to its sides the two long strings of eggs (fig. 8, m); the distal projects between these strings, and constitutes the true posterior end of the animal (fig. 8, n). Attached to the sides of the ventral aspect of this terminal segment are two small papillæ, each of which is provided with a bulbous base.

The alimentary canal commences at the mouth, and extends down the axis of the cavity of the body (fig. 2, 7). It varies somewhat in length. In one specimen we saw that it passed as far as the posterior end of the last thoracic segment, and sent its lateral terminal cœca for a short distance into the lateral terminal lappets of that segment. In another specimen the canal did not pass more than half way down the last thoracic segment. In others, to distances intermediate between these extremes. The mouth opens into the œsophagus, which is a short tube, the walls of which are almost free from dilatations, and the cavity of which is generally empty. The rest of the canal lies in the thoracic segment, and possesses on its sides three pairs of distinct cœca, one pair being situated in each of the last three thoracic segments. The canal between the first pair of cœca and the œsophagus is much wider than the last-named part of the tube. The contracted

portions which connect the different pairs of cœca vary in length in different animals. The canal ends posteriorly in a blunted, rounded cul-de-sac, which lies between the last pair of cœca. This small terminal part RATHKE has described as an intestine; and he considers that it opens externally by an anus. We have looked in vain for such an opening, and must therefore conclude that it does not exist, a conclusion to which CLAUS also had arrived. Moreover, it is quite possible to remove the whole alimentary canal from the visceral cavity, so as to see clearly that it has no connection with the surface except at the oral aperture. The wall of the whole of the canal, except the œsophagus, presents a number of small dilatations, which give to it a very irregular aspect. These are formed by the protrusion of the lining membrane of the gut between small polygonal spaces due to the crossing of the longitudinal muscular fibres of the canal by transverse and oblique fibres. We could not see any indications of transverse markings in these fibres, although CLAUS states that he has seen distinct striations in them. A quantity of (by transmitted light) dark brown granular matter is contained in the canal, especially in the dilatations on its walls.

We cannot say anything definite from our own observations as to the existence or position of a heart, although RATHKE describes as such a thin canal, pointed at both extremities, situated just behind the neck. During an examination of a living specimen, we thought we observed, on one occasion, a movement in the interior of the margin of the fourth thoracic segment, which might be due to a circulatory fluid. RATHKE also describes on the abdominal aspect two nervous cords with proportionately large ganglia, but we were unable to detect any such structures. The animal possesses a very complete muscular apparatus for the movement of the antennæ, hooks, jaws, and limbs; and in addition muscular bundles extend in a longitudinal direction along the ventral and dorsal aspects, especially the former. The fibres possess a delicate but very distinct transversely striped appearance.

A complicated system of branched and communicating tubes is situated on each side, in the visceral cavity of the animal, external to the alimentary canal. This tubular arrangement is the ovary. It extends not only into the lateral processes from the sides of the thoracic portion, but even into the dorsal and ventral median processes and into the thoracic feet, all of which are hollow, and communicate with the interior of the visceral cavity. Some of the tubes commence by rounded, others by finely pointed ends (fig. 10). Many exhibit a very convoluted arrangement. They open on each side into a tube which runs parallel to the intestine, and which is more or less concealed by the gut. This tube is the oviduct, and it extends backwards as far as the proximal segment of the abdominal division of the animal, where it opens. The opening is protected by a chitinic plate, folded inwards on itself towards the mesial line. This folded plate has very much the appearance of a bivalve shell. Just before it opens, it is

joined by a wider tube, the cement-preparing gland (Kitt-organ), which extends for some distance forwards. It was traced almost to the neck; but its exact mode of termination anteriorly could not be satisfactorily ascertained. It lies close to the lateral margin of the thorax, but does not pass into the lappets (fig. 2, h). In the anastomosing branches of the ovary, numerous ova are situated, either in single or double rows. In the great majority of our specimens, a long and somewhat spirally twisted string of ova extends backwards from the genital aperture on each side. The ova are contained in a tough, homogeneous substance, which not only gives to each ovum a specific investment, but forms for the whole a common continuous envelope. It appears to constitute a distinct membranous tube, subdivided into numerous spaces for the ova, but it is probably formed of the secretion of the cement gland, which gives to each ovum a distinct investment as it passes out of the genital orifice, so that, as the number of extruded ova increases, the length of the string necessarily increases. By it they are kept in connection with the body of the female as long as is necessary for their due development. The ova are crowded together in these strings so as to form, not one, but several spirally arranged rows.

Male.—Very minute. Not more than $\frac{1}{30}$th of an inch in length, and is, compared with the size of female, a mere speck (fig. 14). It is parasitic on the female, and is connected by a pair of hooks to the posterior end of the body, close to the genital orifice. Like the former observers, we have never found more than one male attached to each female. In only one case, recorded by Kröyer, have two males been met with on a single female. The place on the body of the female which marks the point of attachment to each other of the sexes, possesses a peculiar structure, which has not been pointed out in any of the former descriptions of the animal. This structure consistsof a papillary projection from the anterior margin of the ventral aspect of the proximal segment of the abdomen (fig. 8, r). In its shape it is very like an intestinal villus, and its surface is covered by elongated, columnar, epithelium-like structures, which, when highly magnified, present in their interior an appearance somewhat difficult to interpret; under some conditions, looking like a nucleus, under others, like a central cavity (fig. 11). The structure of this projection is thus materially different from the ordinary chitinic investment of the female, and offers a substance, softer than it, in which the hooks of the male are embedded. One may conceive, also, that it is through it that the male draws its supply of nutriment. The presence of a nipple-like structure in relation to each genital orifice, would lead one to infer that the male may be connected to either side of the body of the female; and it is probable that he possesses the power of changing his position from one side to the other, so as to impregnate both sets of ova.

The male is elongated in shape, much broader anteriorly than posteriorly.

Contrary to the opinions of RATHKE and CLAUS, we find a very definite segmentation of the body, indicated by constrictions on the margin and surfaces, by which it is divided into as many portions as have already been described in the female (fig. 12, 13). In the male the cephalic part preponderates in size greatly over the thoracic and abdominal divisions. Connected to the head are a pair of antennæ, a pair of hooks, and three pairs of jaws, which, except in some slight modifications of the antennæ, closely resemble in their form, structure, and arrangement, the corresponding parts in the female, though on a much smaller scale. The thorax possesses neither dorsal nor ventral mesial processes, nor lateral lappets. It is divided into four segments, the first and second of which have, in connection with their ventral surface, two pairs of feet. These are more simple than the thoracic feet of the female, and consist of truncated cones with one or more papillæ at their extremities. The abdominal segments are two in number—the proximal having in it the opening of the male genital tube, the distal bearing two well-marked tapering papillæ, as is the case in the female. The alimentary canal lies in the axis. It has an oral opening in front of the bases of the first pair of jaws, as in the female. The canal is dilated anteriorly, and tapers somewhat posteriorly. It possesses no cœca. We could not see an anus. We agree with CLAUS in the description which he gives of the generative organs; but from not having examined the male in the fresh state, we did not observe the spindle-like spermatozoa which he describes. Well marked bands of transversely striped muscular fibres are connected to the jaws and other appendages, and, in addition, dorsal and ventral muscular bands exist.

Larva.—We have found the ova to present very different appearances in the ova-strings of the different animals we have examined (fig. 15). In some the germinal vesicle and spot were visible. In others the contents of the ova consisted of closely-crowded opaque granules, due to segmentation of the yelk. In the more advanced forms, which were distinguishable to the naked eye by the pinkish colour of the strings, further stages in the development might be observed. In some, a distinctly elongated body was contained, consisting of a mass of large yelk granules. In others, a little more advanced, indications of limbs might be seen, whilst at a still later stage the limbs and their divisions were clearly visible. On various occasions we saw distinct movements, not only of the limbs, but of the entire embryo within the ovum. When the embryo has reached this stage it escapes from the ova-string, by a rupture of its thin wall, and of the outer covering of the ovum; the process being probably identical with that which has been described and figured by VON NORDMANN,* in the Ergasilus Sieboldii. We have

* Mikrographische Beitrage, 2d part, p. 13, Pl. II., fig. 3. Berlin, 1832.

seen the embryoes lying in the sea-water still invested by the inner membrane of the ovum. When the embryo finally escapes from the ovum, its shape, structure, and appendages may be more clearly examined. The larva is oval in shape, the broader end being anterior. Length about $\frac{1}{100}$th of an inch. Dorsal surface somewhat convex. A pair of dark-coloured spots is seen at the anterior end. At the posterior a couple of fine hairs projects backwards. An aggregated mass of refracting globules, probably the remains of the yelk, lies in the centre of the body in what may be supposed to be the intestine.

A series of appendages projects beyond the sides of the larva. The anterior pair, evidently antennæ, are three-jointed, the terminal segment possessing two hairs projecting from its free end. Behind the antennæ, and distinctly arising from the ventral aspect, are two pairs of swimming limbs. Each limb consists of a basal and two terminal segments, one in front of the other. Each terminal segment is furnished with hairs; the anterior possesses two at its outer end, the posterior has only one at its outer extremity, but in addition is furnished with three along its anterior margin, each arising from a papilla-like projection. Distinct muscular fibres, transversely striped, were seen in the body of the larva, evidently connected to the basal segment of the limbs. Of mouth and anus we saw no indications. The further development of the larva we had no opportunity of tracing.

PLATE III.—*Explanation of Figures.*

Fig. 1. Lateral view of *Chondracanthus Lophii*. The constriction *t*, separating the cephalic *u*, and thoracic parts, the pair of antennæ, *a*, at the anterior part of the head, and the number and direction of the dorsal and lateral processes, are represented. The two spirally twisted strings of ova project from the posterior part of the animal. The thoracic feet are indicated at *dd*.

Fig. 2. Ventral aspect—*a*, antenna; *b*, hook; *c*, foot-jaws; *dd*, thoracic feet; *ee*, ventral mesial processes; *f*, posterior end of alimentary canal; *g*, abdomen; *h*, cement organ.

Fig. 3. Highly magnified view of the distal segment of one of the antennæ. The continuity of the parenchymatous substance of the hairs with that in the interior of the antenna itself is represented.

Fig. 4. Left hook organ—*i*, terminal hook; *k*, basal portion; *l*, frame. In *A*, the ventral aspect, in *B*, the outer aspect is represented.

Fig. 5. Ventral aspect of cephalic portion. The relations of the antennæ *a*, hooks *b*, mouth *s*, foot-jaws *c*, and the origin of the first pair of thoracic feet *d*, are exhibited. Various muscular bands for the foot-jaws, and portions of the longitudinal muscles are seen.

Fig. 6. Enlarged view of foot-jaws of left side. *C* 1*st*, *D* 2*d*, *E* 3*d* foot-jaw.

Fig. 7. Alimentary canal, with its lateral cœca and small terminal cul-de-sac. The muscular fibres in its wall are represented.

Fig. 8. Abdominal portion—*m*, proximal segment, with its somewhat bivalve-like shape; *n*, distal segment, bearing hair-like papillæ; *o*, ova-strings; *r*, villus-like projection for attachment of male.

Fig. 9. Dissection of abdomen showing the connection of the cement organ *h*, with the investing material of the ova-strings, *o*. The portion of the proximal segment marked *m* in this and Fig. 8, being turned on one side; *p*, longitudinal muscular bands.

Fig. 10. Ramifying ovary, with contained ova, in one of the lateral lappets of the thorax.

Fig. 11. *F*, Villus-like projection to which the male is attached. *G*, Enlarged view of two of the epithelium-like structures. At *r*, Fig. 8, the projection is shown in situ.

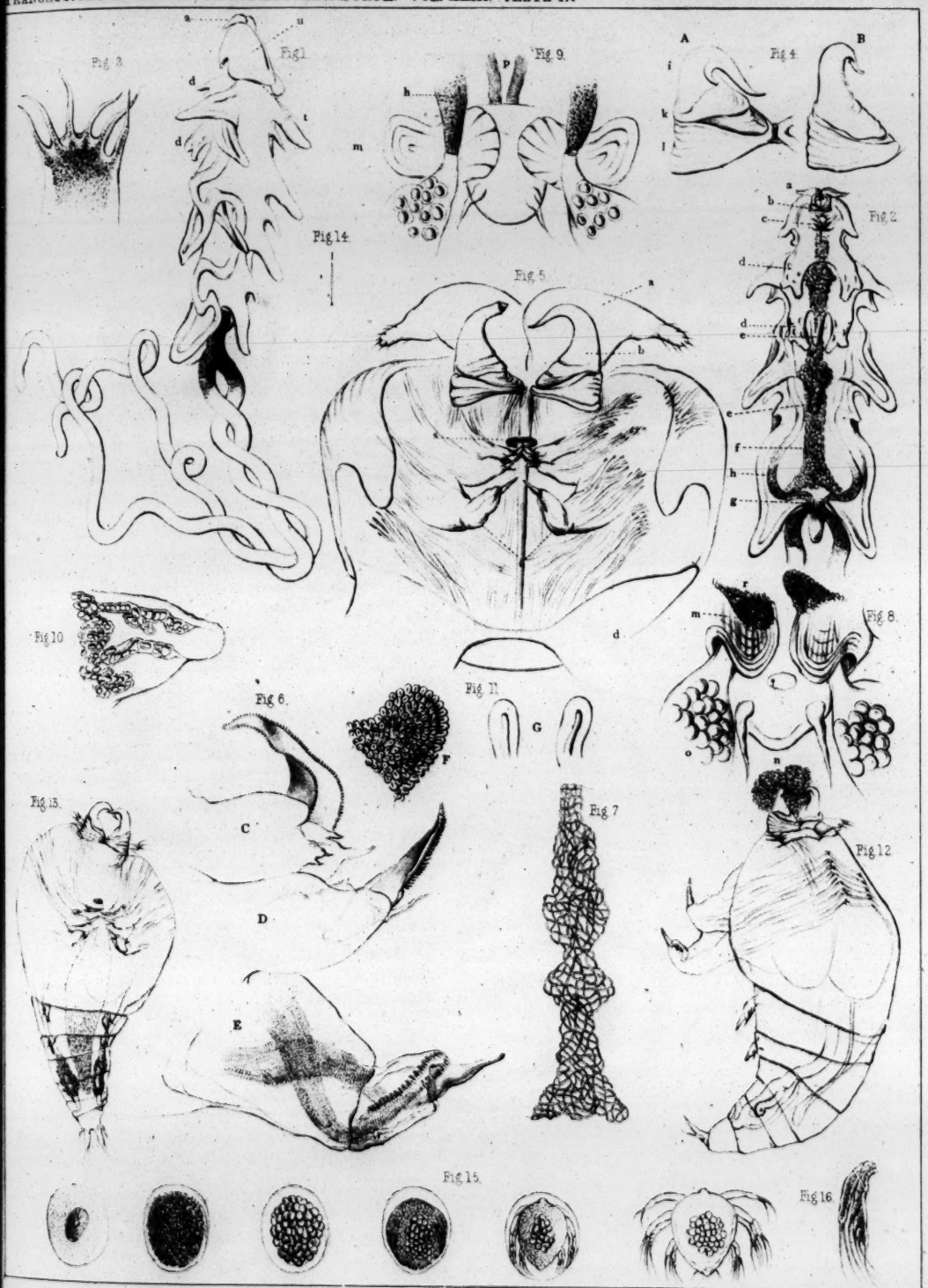
Fig. 12. Side view of male, to the hook organs of which a portion of the villus-like projection is still connected.

Fig. 13. Ventral aspect of male.

Fig. 14. Natural length of male and female without the ova-strings.

Fig. 15. Successive stages in the development of the ova.

Fig. 16. Anterior foot of left side of larva. The division into two terminal segments is seen, with the mode of connection of the hairs to them.


Fig. 3.
Fig. 1.
Fig. 9.
Fig. 4.
Fig. 14.
Fig. 5.
Fig. 2.
Fig. 10.
Fig. 6.
Fig. 11.
Fig. 8.
Fig. 13.
Fig. 7.
Fig. 12.
Fig. 15.
Fig. 16.

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VIII.—*On the Structure of Lerneopoda Dalmanni, with Observations on its Larval Form.* By WILLIAM TURNER, M.B. (Lond.) F.R.S.E., and H. S. WILSON, M.D., Demonstrators of Anatomy. (Plate IV.)

(Read 7th April 1862.)

The first example of this, apparently little known, species of Parasitic Crustacean appears to have been noted by Professor OTTO.* The celebrated Swedish naturalist RETZIUS was, however, the first to give, in 1829, an anatomical description of it.† He named it *Lernœa Dalmanni*. His description was accompanied by several figures, which, though in many respects imperfect, enable one to recognise the chief external characters of the animal. He found three specimens at Christian Sound, in the nasal cavity of Raia Batis. VON NORDMANN‡ obtained from RUDOLPHI the specimen discovered by OTTO, but it was so injured, that he adopted, in his account of the anatomy of the animal, the description of RETZIUS. Some years afterwards, in 1836, KRÖYER‖ added it to the Danish fauna. He states that he obtained two specimens from the nasal chamber of a skate brought to him by a fisherman from Aal-back, and that specimens from Iceland had been for several years in the possession of the Natural History Society. As naturalists had now begun to subdivide the old Linnean genus Lernæa into various genera, KRÖYER added this animal to the genus Lerneopoda of DE BLAINVILLE, and, continuing its specific name, termed it *Lerneopoda Dalmanni*.

Since the time of KRÖYER but little attention appears to have been bestowed on this parasite by systematic writers. MILNE EDWARDS§ mentions it only briefly, and is inclined, from the elongated and cylindrical form of the cephalic part, and from the development of two processes from the ventral aspect of the posterior end of the body, to place it in the genus Brachiella.

It does not appear to have been, as yet, recognised as a British species, for no mention of it is made in the systematic works of BAIRD and GOSSE. The fortunate detection by one of us (Dr WILSON) of several specimens, early in the present year, in the nasal cavities of more than one skate, caught by the Newhaven fishermen, has enabled us to add it to the British fauna, and to investigate its anatomy.

* Mikrographische Beiträge Von A. v. Nordmann. Berlin, 1832.

† Kongl. Vetenskaps Acad., Handlingar. Stockholm, 1829. P. 109. Frorieps Notizen, vol. xxix. N. 617, p. 6.

‡ Op. Cit. p. 139.

‖ Naturhistorik Tidskrift, vol. i. p. 264. Okens Isis, 1840, p. 746.

§ Hist. Nat. des Crustacées, vol. iii. p. 516.

Female (figs. 1, 2).—This is evidently one of the largest of the Lerneadæ. It presents decided indications of being divided into two great segments,—an anterior, or cephalo-thoracic, and a posterior or abdominal. The division is indicated by a well-marked constriction or neck. Immediately in front of this neck a pair of long arms arise from the sides of the cephalo-thorax.

The cephalo-thorax in a fully grown specimen is $\frac{4}{10}$ths of an inch in length. It projects almost at a right angle from the anterior extremity of the abdomen. It is elongated, and somewhat compressed laterally. On its dorsal surface, about the junction of the anterior and middle thirds, a pair of antennæ is situated (figs. 3, b). Their bases are partially concealed, and connected together by a crescentic fold of the chitinic integument of the animal, the convexity of which fold is directed backwards. Each antenna is 3-jointed: the basal segment short and broad; the second much longer; the terminal smaller, and possessing a pair of hooks at its free end (fig. 5).

In front of the antennæ, and therefore close to the anterior extremity of the head, a complicated buccal apparatus is met with (fig. 3, c, fig. 6). Situated in the middle line is a short conical snout, at the free extremity of which a rounded oral aperture exists. The animal appears to possess the power of retracting and projecting this snout at pleasure. On each side of the snout is a short stump-like process, slightly bifid at its extremity. For the due examination of the structures about the mouth, the higher powers of the microscope are necessary. The buccal apparatus consists of an oral aperture, of two lip-like structures, and of a pair of jaws. The lower lip is strengthened by a peculiar arrangement of chitinic bands, which have been represented in fig. 9. Projecting not only from the margin, but also partially from the inner surface of this lip, are a number of fusiform bristle-like papillæ. On the inner surface of the central part of this lip several very delicate, faintly transversely-barred lines pass downwards and backwards as far as a chitinic bar, which stretches across the lip from margin to margin. The upper lip (fig. 8) consists of two chitinic concavo-convex plates, which fit into each other, their concave aspects being directed to the buccal cavity. The inner plate is shorter than the outer, and bears a row of long slightly undulating, transversely striped, rod-like structures, fringed at their free ends. The outer plate is covered at the margin, and for some distance on its dorsal aspect, by sharply pointed, conical, bristle-like papillæ. The lips are supported at their bases on a chitinic plate, more immediately continuous with the plates of the lower lip, and to which the upper lip is apparently connected by a moveable joint. This plate is so arranged as to be folded upon itself in such a manner as to enclose tubular spaces (fig. 6, b), in which some of the muscles of the buccal apparatus are included.

When the lips are drawn asunder, so as to expose the buccal chamber, a pair of elongated scythe-like jaws (mandibles) is seen (fig. 9). These are attached

laterally close to the angles of junction of the lips. They lie obliquely across the cavity, and therefore cross each other. In some of the specimens the free ends of these mandibles projected externally through the oral aperture (fig. 7). Deeply serrated strong teeth, about six in number, with intermediate smaller teeth between the three terminal ones, are placed along the concave posterior margin of the unattached extremity of each jaw. Each mandible is articulated externally to the chitinic plate which supports the lips. The muscles which move the jaws do not present any distinct transverse striation. They are marked by delicate longitudinal striæ, which resemble nuclei in their appearance.

Connected laterally to the outer aspect of the upper lip is a very small projecting structure, which KRÖYER has described as a touch organ, and which may evidently be considered to be a labial palp (fig. 8, a). It consists of two segments, from the terminal one of which three papilla-like hairs project. The basal segment has, along its line of articulation with the terminal, a short hook-supporting tubercle, and from its outer surface, about half way towards the base, there is an elevation fringed with very short bristles.

The stump-like processes, between which the snout, bearing the mouth, lies, have been termed by KRÖYER the second pair of antennæ (fig. 6, d, fig. 10). They correspond very nearly, both in structure and relation to the mouth, to the appendages which V. NORDMANN has described and figured in *Achtheres percarum* as the upper jaws, or metamorphosed first pair of feet. Each of these structures is segmented and laterally compressed, and arises by an elongated base, which is not merely on the same line with the base of the snout, but extends forwards beyond it for a short distance towards the anterior extremity of the cephalo-thorax. Each possesses a bifid free extremity, the posterior division of which, smaller than the anterior, is armed with a well-marked terminal hook. Along its posterior margin are two elevations, studded with short, blunt, cylindrical bristles (fig. 10, a). Immediately above the lower of these elevations is a smaller space covered with conical hairs. The anterior division, very much larger than the posterior, is thickly studded, especially on its outer surface, with a corresponding bristle-like arrangement (fig. 10, b). Springing from the anterior and inner part of each of these modified feet is a segmented palp-like structure, set with three or four conical papillæ at its free extremity (fig. 10, c).

At the inferior margin of the anterior extremity of the cephalo-thorax is a somewhat pendulous nipple-like structure, which has been named by RETZIUS the chin (figs. 4, b). It consists simply of a projection of the integument, which can be retracted or protruded at pleasure.

A pair of elongated cylindrical arms arises from the sides of the cephalo-thorax immediately in front of the constricted neck (fig. 1, c). When fully extended, each arm measures an inch, but it can be contracted by the animal at least one-half. The arms pass almost vertically upwards in the direction of the long axis of the

abdomen. They lie side by side, and almost parallel to each other. Each arm ends superiorly in an expanded structure, concave on its upper aspect, which performs the office of a clasper. The adjacent surfaces of the two claspers are flat, and, when the arms are in their normal position, these surfaces are accurately adapted to, but not continuous with each other, so that they can be drawn asunder without any difficulty. The arms, therefore, cannot be regarded as blended together at their extremities. Lying in the concave upper surfaces of the apposed claspers is a curved cartilaginous-like bar (fig. 2, a), the convex outer surface of which is closely embraced by them. The bar has in its interior a distinct cavity. Projecting from the surface of the bar which is in apposition with the claspers, is a small papilla, along the axis of which the central cavity of the bar extends. Between the superior margins of the flattened apposed surfaces of the claspers a slight depression exists, into which the papilla of the bar fits. Two small oval openings exist at the bottom of this depression, one belonging to each clasper. Each communicates with a long and slender tube, which passes through the substance of the clasper to be continuous with the central canal of the cylindrical part of the arm. The substance of the bar, when examined microscopically, without the addition of any reagent, appears to be structureless. After digesting thin slices in ether, and subsequently boiling them for some time in acetic acid, a very beautiful cellular structure is perceived. These cells are about the size of primordial cartilage cells. In them the nucleus is elongated, and between it and the cell-wall a delicate concentric arrangement is seen. There is evidently a difference between the chemical composition of the bar and the chitinic integument of the animal, for on steeping a specimen for some hours in a chromic acid solution, whilst the colour of the latter was very slightly affected, that of the bar was changed to a dark brown, almost black.

Both the claspers and the cylindrical portions of the arms contain powerful muscles, by the contraction of which the arms can be very much shortened, at which times they have a very crenulated appearance. These muscles are transversely striped, and are arranged both in longitudinal and circular bundles. A canal extends along the centre of each arm, which communicates at its root with the general cavity of the body. This canal is irregularly subdivided by connective tissue bands passing across it.

It is by means of the arms that this crustacean attaches itself to the skate on which it is parasitic. When examined *in situ*, the arms could be traced passing along the space which separates two of the nasal laminæ from each other. They then enter a canal of calibre sufficient merely to contain them. This canal dilates at its end into a comparatively large space, lined by a distinct smooth vascular membrane. In this cavity the claspers, with the bar which they embrace, are situated. The bar lies transversely, its long axis corresponding to that of the space in which it is situated; and as the canal, in which the cylindri-

cal portions of the arms lie, is at right angles to the long axis of this space, it is impossible to draw the bar and claspers out without dissecting away the wall of the cavity.

We have already mentioned, that M. EDWARDS has seen reason to think, that owing to the elongated cephalo-thorax, and the existence of posterior abdominal appendages, this parasite possesses characters which hardly permit it to be classed along with the other known Lerneopoda. To these we may add the very important one, that the arms are not united at the tip. For although they are in close apposition by the flat surfaces of their clasper-like terminations, yet the parasite, when removed from the fish to which it is attached, can spontaneously withdraw one or other arm, or both, from the transverse cartilaginous-like bar, so as to separate the claspers completely from each other. After withdrawing the claspers from the bar, the animal does not appear to possess any power of re-attaching them to it. This character of ready separation of the ends of the arms from each other would also prevent us from placing the animal, as has been suggested by M. EDWARDS, in the genus Brachiella.

Projecting from each side of the cephalo-thorax, immediately in front of the root of each arm, is a well-marked bulb-like protuberance, noticed both by RETZIUS and KRÖYER, and termed by them the eye-like spots, although, from their colour and structure, they do not consider them to be eyes (fig. 3, a). Each has the appearance of a segment of a sphere. Through the semi-transparent integument of the most prominent part, a quantity of reddish-brown granules, aggregated in elongated masses, may be seen. Low magnifying powers also enabled us to detect a very peculiar rod-like structure, connected apparently to the deep surface of the eye-like spot. It commences in a slightly dilated bulb-like part, which passes backwards and outwards, around the base of the arm on its own side, and terminates in a dilatation similar to that by which it commences. The exact structures to which it is connected at its two ends we cannot say with certainty; but it is probable, from its position, that it may act as a sling for the support of the base of the arms. The rod has a diameter of $\frac{1}{3\,1\,0}$th of an inch, the bulb of $\frac{1}{1\,8\,2}$th. Its structure is characteristic, so that it can at once be distinguished from the surrounding parts. It possesses an axial portion, which is about one-third the diameter of the entire rod. Under high magnifying powers this presents a corrugated aspect, which reminded us of the well-known appearance of the coagulated medullary sheath of a nerve fibre. Surrounding this axial structure is a very transparent substance, which has many of the microscopic characters of chitine.

The abdomen is $\frac{1}{10}$ths of an inch long, $\frac{1}{16}$ths broad. It ends posteriorly in a rounded elevation on each side, between which is a depression, so that it possesses an inverted heart-shaped form (fig. 1, c; fig. 2, b). It is slightly convex on the dorsal, almost flat on the ventral aspect. It has an imperfectly defined segmented appearance, owing to the existence of three circular depressions in the chitinic in-

tegument, by which it is divided into four segments. These external depressions correspond to folds which project into the visceral cavity, and to which powerful muscles are connected. When these muscles are contracting, the segmentation is more manifest. The first segment lies immediately behind the roots of the long arms, and possesses on each side a bulging, corresponding in appearance, but smaller, to the eye-like spot on the cephalo-thorax, already described. The third segment has a similar pair of protuberances, placed, however, more on the ventral surface. The fourth segment is as large, or even larger than the conjoined first three segments. In the median line of its ventral surface, close to the posterior margin, is the longitudinal slit-like anal aperture. This opening is bounded on each side by a well-marked fold of integument (fig. 12, a). Attached to the outer aspect of each fold is an elongated cylindrical process, which projects downwards, and is slightly curved. It is a little broader at the free than the attached end. Each of these posterior abdominal appendages is $\frac{1}{10}$ths of an inch long. Two elongated ova strings project also from the ventral surface. They spring from it posterior and external to the anus.

The intestinal canal lies in the axis of the cavity of the abdomen. It is retained in its position by numerous delicate bands, which pass from it to various parts of the inner surface of the wall of the cavity. It is of almost uniform calibre throughout, and presents a crenulated appearance. Distinct muscular fibres, arranged longitudinally and circularly, enter into the formation of its wall. It terminates posteriorly in the slit-like anal aperture. At the anterior end of the abdomen the canal bends, so as to pass into the cephalo-thorax, along the axis of which it extends. It makes a slight bend towards the dorsal aspect of the anterior extremity of the cephalo-thorax, so as to open at the oral aperture. The canal is more dilated in the cephalo-thorax than in the abdomen. It communicates with the buccal chamber, not by a broad expanded opening, but through a chink-like fissure, between two plates of chitine.

Lying in the visceral chamber external to the alimentary canal, is a quantity of what appears to the naked eye to be merely brown granular material. From its position, colour, and general appearance, it probably represents the liver in this creature. When highly magnified, it looked like gland tissue, for it consisted of vesicular dilatations, or saccules, apparently containing cells and granules. Special collections of a similar gland-like substance are to be seen at the roots of the arms, and extending for some distance along their central canals, in the eye-like spots, the lateral dilatations of the abdomen, and the posterior abdominal appendages.

The ovaries, two in number, are situated in the abdominal cavity, and are confined to the fourth segment. They lie on each side of the intestine. Each consists of a ramified system of tubes, which communicate with a duct. This duct runs almost parallel to the intestine. It opens on the ventral aspect of the fourth seg-

ment, close to the posterior margin. The orifice is supported by chitinic bands. The cement organ consists of a simple delicate tube. It commences in the anterior part of the abdomen, and passes backwards close to the lateral wall. Its position may be frequently recognised through the integument without dissection, especially in the living animal. It joins the ovarian duct close to the genital orifice. In all the animals we examined the ovaries were distended with ova. They formed opaque, white masses, readily seen through the semi-transparent integument of the abdomen.

The ova-strings are $\frac{5}{16}$ths of an inch long, cylindrical in form, of greater circumference at their attached than free ends. They are slightly curved, the concavities being directed inwards. The ova lie in longitudinal rows, with a spiral tendency. When a transverse section is made through an ova-string, the ova at the periphery are seen to be very elongated, whilst those in the centre are nearly circular. When the ova-strings rupture, in order that the ova may be discharged, they burst along the inner concave aspect. The margins along the line of rupture become everted, and thus the exposure of the ova in the interior of the string is facilitated.

The animal possesses a very powerful muscular system. The largest bands are situated on either side of the dorsal and ventral mesial lines, being attached to folds of the chitinic integument. The buccal apparatus, antennæ, and arms, have special muscular arrangements. In many of our dissections the complex structure of striped muscular fibre was very beautifully illustrated. Resolution of the fibre in some cases into fibrillæ, in other into discs, not unfrequently taking place.

We have looked carefully for a nervous system, and think that we have seen appearances indicative of its existence. We slit open the animal along the dorsal mesial line, and carefully removed the ovaries and intestinal canal. On examining the internal surface of the abdominal wall with a magnifying power of 200 diameters, collections of cells could be seen in many places. These were especially manifest in the space between the roots of the two arms. These cells had many of the characters of nerve-cells, such as delicate outlines, granular contents, aud connecting processes. The processes from many cells were evidently two in number, though, from the close manner in which the cells were crowded together, it was not possible to distinguish in some more than one process, and in others again none could be observed. The investigation of the arrangement of the nervous system in these creatures is evidently attended with considerable difficulties, partly owing to the delicacy of the structures, and partly on account of the strong ventral muscular bands and the gland-tissue surrounding the intestinal canal, the presence of which interferes greatly with its due examination.

If the female be carefully separated from the skate, and placed in clean sea-water, we have found it possible, by occasionally changing the water, to keep the

animal alive for three weeks. If, in the act of separation, the bar be removed from the claspers, death takes place at an earlier period.

Male.—We have not succeeded in finding the male of this species. Following the rule pursued by these parasites, we had expected to have seen it attached to the body of the female, but we have carefully searched the different specimens we have obtained without meeting with it. RETZIUS makes no mention of the male. KRÖYER thinks that he found on one of his specimens a male attached at the anus. He describes a creature "about one-third of a line long, with somewhat of a crustacean form, with two 2-jointed antennæ, a 3-jointed thoracic portion, a curved tail, and two strong hooked feet. The head appeared to present the trace of an eye. He can say nothing further about it."

Larva.—Neither RETZIUS nor KRÖYER have given any description of the larva. In two of our specimens the ova were in such a stage of development that we were enabled to examine the form of the larva. In some of these ova the larva could be studied at the stage immediately preceding the rupture of the ovum; in other cases the free larvæ were obtained. Length of larva, $\frac{1}{17}$th of an inch; breadth, $\frac{1}{60}$th. When viewed from dorsal aspect, its shape appears ovoid (fig. 13). When a profile view is made the ventral surface appears nearly flat, the dorsal very convex (fig. 14). A pair of antennæ project from the ventral surface close to the anterior margin. They possess indications of being three segmented, the terminal one having at its free end a pair of long hairs. The limbs consist of two pairs; the anterior pair arises from the ventral surface, close to the antennæ. Each limb is bifid (fig. 15, A). The upper branch bears four or five very long hairs at its extremity; the lower, in addition to two long hairs at its free end, has a spinous hook projecting from it. The posterior pair arises from the ventral aspect, nearer the posterior than the anterior end of the larva. Each limb is shorter and thicker than the anterior, and in none of the specimens examined could we see, on a dorsal view, their extremities projecting beyond the lateral margin of the larva. Each divides into two branches, one somewhat larger than the other (fig. 15, B). A strong spinous hook, which appears to be capable of retraction within a sheath, arms the branches of each limb. In addition to these appendages the larva possesses a somewhat complicated arrangement, springing from the ventral aspect, close to the posterior margin. It consists of a triangular prolongation, folded over the posterior part of the convex dorsal aspect, so that the apex only can be seen when the dorsal surface is looked down upon. It gives off laterally three pairs of processes, each of which has connected to it two lappets, bearing long pinnate hairs (fig. 16). It terminates by a lappet-like process, which bears short, stiff hairs. The antennæ, limbs, and tail-like appendages, have transversely striped muscular fibres connected to them. The

intestinal canal commences behind the antennæ, by a trumpet-shaped mouth. It passes backwards and upwards towards the convex dorsal aspect, then curves downwards and forwards towards the ventral aspect, and ends abruptly close to the oral opening. In the cavity of the body there are numerous collections of oil drops, especially towards the posterior end of the larva. The eye-spots are situated on the dorsal aspect, not far from the anterior margin. Two large branched masses of dark-red granules may be seen, one close to the eye-spots, the other farther backwards; and in the free larva these masses are connected by long streaks of reddish pigment.

The further development of the larva we have had no opportunity of observing.

ADDENDUM, *June 24th.*—When we read our paper before the Royal Society, none of the more typical and best-known species belonging to the genus *Lerneopoda* had come under our notice. Since that time, through the liberality of Mr ROBERT BROWN, we have had the opportunity of examining three specimens of the *L. elongata*, which were obtained by him attached to the eye of a Greenland shark (*Scymnus borealis*) caught in Ponds Bay, Davis' Strait, in the summer of last year.

We have especially directed our attention to the distal extremities of the arms, with the view of making a comparison between the mode of attachment of this species to the cornea of the shark, and that of *L. Dalmanni* to the skate. The arms of *L. elongata* taper abruptly at their distal ends, and are connected to a small, rounded, horny, or chitinic disk. This attachment is evidently of a very close and intimate description, for in attempting to separate them from it, the substance of the arms gave way, rather than permit the connection between them and the disk to be severed. We could not say with absolute certainty whether the structures composing the arms were anatomically continuous with that of the disk, but there were appearances which led us to suppose that the chitinic investment of the arms was continuous with that of the disk. The arms themselves are evidently not united at their tips, except through the medium of the common plate to which they are both connected. In our specimens we saw nothing to lead us to suppose that the arms were inserted into the substance of the cornea, as is stated by BAIRD* to have been the case in the specimen he examined, for the disk was undoubtedly attached to the surface of the cornea. Of the two eyes of the shark obtained by Mr BROWN, one had two parasites connected to it, the other only a single one. But each cornea had, in addition, a number of circular

* British Entomostraca, p. 334.

markings on its outer surface, evidently the scars which indicated the former attachment either of the same, or other parasites. A similar appearance was described and figured by GRANT in his specimen.* It will thus be seen that there are considerable differences in the mode of attachment of the *L. elongata* and *Dalmanni*. These differences are doubtless due to the varying nature of the localities in which they are met with. The *L. elongata*, being adherent to a flat surface, has connected to the ends of its arms a sucker-like disc; whilst the *Dalmanni*, being to some extent buried in the substance of the skate, has the clasper-like terminations of its arms attached to a transverse bar, which is lodged in a special cavity.

The comparison which we have been enabled to make between the *L. elongata* and *Dalmanni* has convinced us that the differences existing between them are so great, that the latter animal ought no longer to be included in the genus in which it is at present placed. The mode of termination of the arms at their distal ends presents such striking peculiarities, that, conjoined with the elongated head, the flattened, inversely heart-shaped abdomen, and the existence of posterior abdominal appendages, we consider it ought to constitute a new genus.

* Brewster's Edin. Journal of Science, 1827, p. 150.

PLATE IV.—*Explanation of Figures.*

Fig. 1. Profile view of Female, a little below natural size; *a*, cephalo-thorax; *b*, "eye-like spot;" *c*, prehensile arms terminated by apposed claspers (bar not indicated); *d*, abdomen; *e*, abdominal appendages; *f*, ova-strings.

Fig. 2. Ventral aspect of female; cephalo-thorax seen raised above its normal position; arms distinct up to the apposed claspers, which embrace the transverse bar, *a*; *b*, abdomen, the four segments faintly indicated. At its posterior extremity may be seen the integumentary folds bounding the anal fissure, and having connected to them the abdominal appendages. Close to these are the ova-strings.

Fig. 3. Dorsal aspect of cephalo-thorax, enlarged; *a*, bulb-like protuberance ("eye-spot") *b*, antennæ (their connected bases are indicated); *c*, buccal apparatus, consisting of a central conical mass bearing the mouth, by the sides of which are the two lateral, short, stump-like processes (modified feet); *d*, integumentary fold for protecting the oral apparatus.

Fig. 4. Profile of cephalo-thorax, enlarged; *a*, dorsal aspect, with antennæ, base of modified feet and protective fold indicated; *b*, projectile process ("chin").

Fig. 5. Antennæ; dorsal aspect magnified.

Fig. 6. Buccal apparatus; ventral aspect magnified; *a*, dorsal and ventral lips, containing between them the oral slit; *bb*, chitinic arrangement supporting the lips, and affording protection to their muscles; *c*, labial palp; *d*, stump-like process (modified foot), to which a palp-like structure is connected.

Fig. 7. Oral aperture, enlarged; *a*, dorsal (upper) lip, presenting an inner plate bearing numerous rods, and an outer, fringed with papillæ; *b*, ventral (lower) lip, also bearing papillæ. Close to the latter, and projecting for a short distance through the buccal orifice, are seen the jaws.

Fig. 8. Portion of upper lip, greatly magnified; *a*, labial palp.

Fig. 9. Lower-lip, greatly magnified, and having connected to its base a pair of jaws; *aa*, chitinic masses for supporting the upper lip.

Fig. 10. Stump-like process or modified foot, magnified; *a*, posterior hook-bearing terminal division; *b*, anterior portion studded with short bristles; *c*, its palp.

Fig. 11. Portion of upper lip magnified, to which is connected a chitinic arrangement, presenting a chink ("pharyngeal fissure"); *a*, commencement of the œsophagus.

Fig. 12. Posterior extremity of the abdomen, enlarged; *a*, anal folds, to which the abdominal appendages are attached, and between which the slit-like anal aperture lies; *b*, ova-strings passing from the genital orifices.

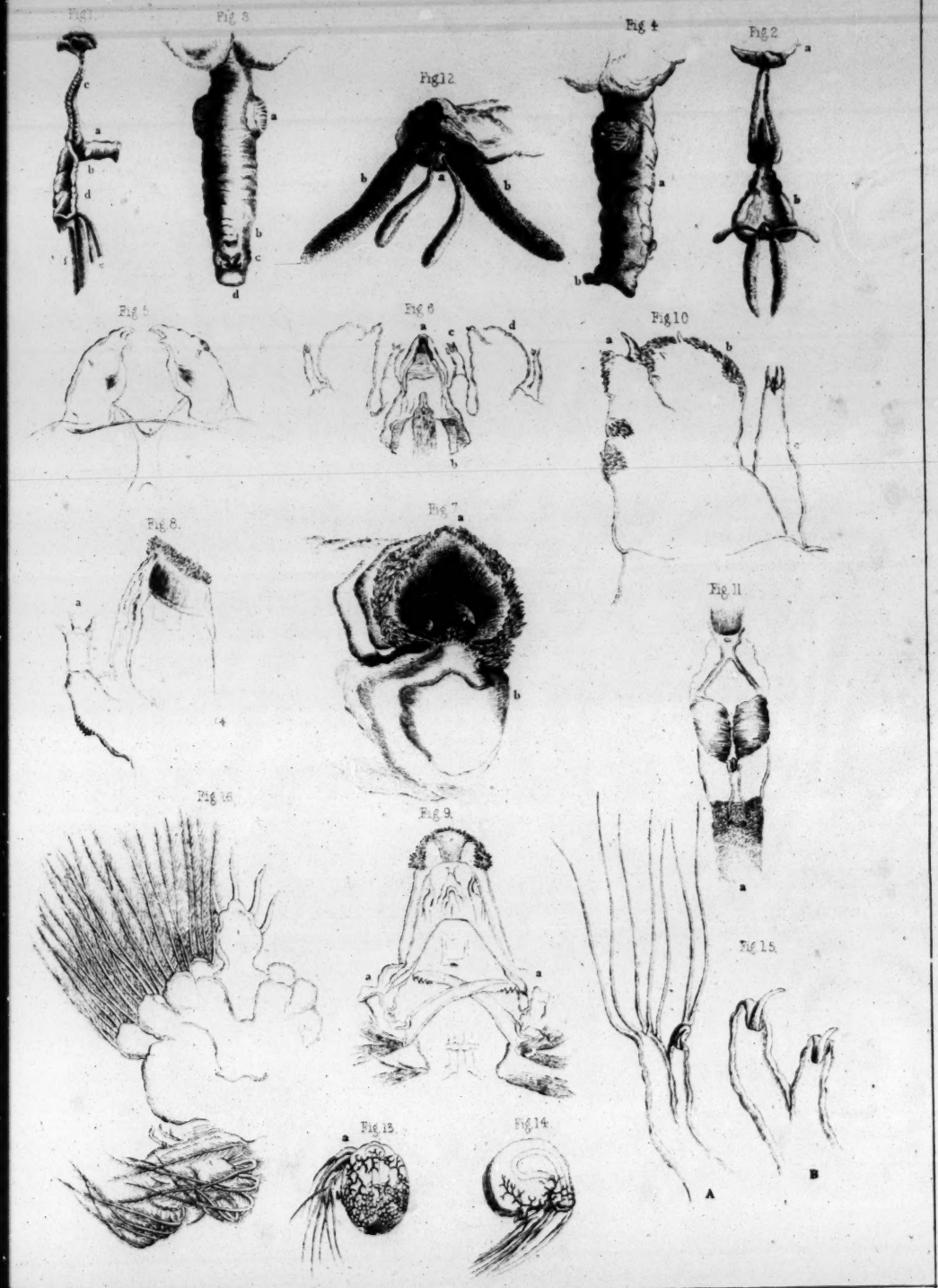
Fig. 13. Dorsal aspect of the larva; *a*, antenna; *b*, first pair of limbs.

Fig. 14. Profile of the larva; the intestinal canal, bent upon itself, is indicated.

Fig. 15. *A*, anterior limb of the larva.

B, posterior limb of the larva.

Fig. 16. Caudal appendage of the larva. One of the lappet-bearing masses has been raised, so as to show the opposite aspect.



S. Wilson, M.D., Delt.

P. Schenck, 11 Rl Exchange, Edinr

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IX.—*On the Deflection of the Plummet due to Solar and Lunar Attraction.* By Edward Sang, Esq. (Plate V.)

(Read 21st April 1862.)

As the means for making observations on the heavenly bodies become more and more exact, astronomers are compelled to introduce new refinements into their calculations; new inequalities are discovered, and the computation of those whose sources are already known has to be carried to a greater number of approximative steps.

Discussions on the amount of solar parallax, of aberration and of nutation, are now carried on to the third and fourth decimal fraction of a second; with such a refinement of computation, it seems almost impossible to proceed too far in the refinement of theoretical deductions, and on that account, it may not be inopportune to discuss the influence which the sun's and moon's attraction exert upon the direction of the plumb-line.

The strict and symbolical investigation of this subject presents no difficulty, and offers no point of any interest to the analyst; it will therefore be sufficient to exhibit the matter in what may be called a popular light; the more so, that the reasoning will thereby lose none of its strictness.

The direct attraction of the sun upon a body on the surface of the earth amounts to about the 1600th part of gravitation, and a pressure equal to it, applied horizontally, would deflect the plummet by an angle of 128 seconds.

The statement, that a body weighing one pound, that is, a body attracted to the centre of the earth by 7000 grains, is also attracted to the sun by $4\frac{1}{3}$ grains, is rather startling. We might expect that direct evidence of so strong an attraction should have been afforded by mechanical phenomena.

The earth's attraction is at once manifested to us by the pressure which every substance exerts upon that below it, or by the rapidity with which it descends, when left free to move. But there is no solid matter intervening between us and the sun, to resist gravitation towards it, and we are unable to perceive the motion sun-wards, because all surrounding bodies partake of the same motion.

In one second of time the earth is deflected from its rectilineal course by about one-eighth part of an inch; now, in that time, its linear motion is about 17 miles; and we may obtain some remote idea of the curvature of the earth's orbit, by imagining a circle which deviates from its tangent by one-eighth part of an inch at the distance of 17 miles from the point of contact.

The centre of the earth is carried round the sun in an orbit due to the attrac-

tion upon a material point situate at that centre, and all bodies on the earth partake of that motion; but the sun's attraction varies inversely as the square of the distance, and is greater for a body on the near side, less for one on the farther side of the earth. Hence a body on the near side would tend to describe an orbit more curved, one on the farther side an orbit less curved, than the earth's actual orbit; and in either case, there is a tendency to augment the distance between the body and the earth's centre. Thus, as is well known to every one who has studied the theory of the tides, the sun's attraction tends to raise the ocean on the farther as well as on the near side of the earth.

The perceivable part of the sun's attraction is therefore the difference between the attraction on a body placed at the earth's centre and that on a like body placed at the surface; or, more strictly, is the resultant of the sun's actual attraction, and of a repulsion equal in intensity, and parallel in direction to the attraction which the sun exerts upon a like body placed at the earth's centre.

If we put D for the distance of the sun, R for the radius of the earth, and α for the sun's zenith distance, the intensity of the attraction is almost proportional to

$$(D - R\cos\alpha)^{-2} = D^{-2} + 2D^{-3}R\cos\alpha + 3D^{-4}R^2\cos\alpha^2 + \&c.$$

Now, the first term of this series represents the attraction at the earth's centre, wherefore the remaining terms

$$D^{-2}\left\{2\frac{R}{D}\cos\alpha + 3\frac{R^2}{D^2}\cos\alpha^2 + \&c.\right\}$$

represent the disturbing influence. The value of the fraction $\frac{R}{D}$ is about $\frac{1}{24{,}000}$th, and even the first term of the series amounts to a very small quantity; wherefore, neglecting the succeeding terms, we may hold that, for all practical purposes, the formula

$$D^{-2} \times 2\frac{R}{D}\cos\alpha,$$

represents the amount of the disturbing influence exerted in the direction of the attracting body. This influence is zero when the sun is on the horizon, and is greatest when the sun is in the zenith or nadir.

But when the sun is in the zenith or nadir, the disturbing influence is exerted directly upwards, and cannot tend to deflect the plummet; in fact, the deflecting tendency is proportional to the sine of the zenith distance as well as to the above quantity, so that the tendency to deflect the plummet is finally represented by

$$D^{-2} \times 2\frac{R}{D}\cos\alpha \cdot \sin\alpha = D^{-2} \times \frac{R}{D}\sin 2\alpha.$$

Hence, the plummet is most drawn aside when the attracting body is 45° above or below the horizon.

For the earth's mean distance D^{-2} may be taken to represent 128″; and,

therefore the deflection of the plummet due to the sun's attraction may be stated as

$$\frac{1''}{186} \cdot \cos 2\alpha(\odot).$$

The moon, though much nearer to us than the sun, is yet so small in comparison, that the effect of a pressure equal to her attraction would only derange the plummet through one second. In this case, however, the ratio of D to R is only as 60 : 1, and therefore the deflection due to the moon's influence is

$$\frac{1''}{60} \cdot \sin 2\alpha(\rho)$$

or three times that due to the sun.

These expressions represent almost exactly the deflections corresponding to the mean distances of the two luminaries; they would, if we were to seek for extreme precision, need to be varied in proportion to the third power of the horizontal parallax.

The deflection of the plummet takes place in the vertical plane passing through the attracting body; and the actual position of the apparent nadir point may be obtained by referring it to co-ordinate axes drawn, the one from north to south, the other from east to west through the true nadir.

Putting x for the ordinate measured southwards, y for that measured westwards, λ for the north latitude, δ for the north declination, and h for the hour angle, we easily obtain

$$\begin{aligned} x &= -\sin 2\lambda \cdot \sin \delta^2 - \cos 2\lambda \cdot \sin 2\delta \cdot \cos h + \sin 2\lambda \cdot \cos \delta^2 \cdot \cos h^2 \\ y &= \qquad\qquad + \sin \lambda \cdot \sin 2\delta \cdot \sin h + \cos \lambda \cdot \cos \delta^2 \cdot \sin 2h, \end{aligned}$$

the values being in terms of the maximum deflection assumed as unit.

These form the equation of an elliptic epicycloid produced by carrying the centre of one ellipse round the circumference of another, the periodic times being 24^h and 12^h respectively, and the motions being such as to generate areas uniformly round the centres. Like all epicycloids, these curves change their appearance with every change in the constants; they have distinct phases, according as the observatory is within the arctic circle, above latitude 45°, on the parallel of 45°, below latitude 45°, intertropical, or on the equator.

These phases are figured in the accompanying drawings, in which the hour angles are marked from the meridian; and which explain, better than any words can, the remarkable changes which the motions of the nadir point undergo as the attracting body changes its declination. The line AB represents $\frac{1''}{60}$ for the lunar deflection, and $\frac{1''}{186}$ for the solar. It may be well to advert to some of the leading features.

When the observatory is within the arctic circle, and when the attracting

body does not set, the apparent nadir describes a circuit round the true. As the declination decreases, the orbit shows a tendency to form a cusp towards the true nadir, and when the moon or sun comes just to graze the horizon the cusp is formed. When the declination becomes so low that the moon sets, the orbit has a convolution inwards; and as the declination approaches to zero, the two sweeps of the orbit coalesce into an ellipse whose circumference is gone over twice daily. So soon as the moon passes to the south of the equator, what may be called the day part of the orbit takes the place of the night part for the corresponding north declination; so that the same diagrams answer, merely changing the hour marks by 12 hours.

For all places above 45° of north latitude the day is larger than the night part of the curve when the moon is north of the equator. For places on the parallel of 45° the two branches of the curve touch each other on the meridian. whatever be the declination, and for places below 45°, the intersection with the meridian are, as it were, interchanged.

For places on the equator, the orbits are always varieties of the hour-glass curve, merging into a simple line when the declination is zero.

There are two ways in which we may attempt to exhibit these phenomena. One is to prepare a spherical level ground to such a large radius of curvature as that the second may be divided into several hundred parts. The motions of the air-bubble will be opposite to those of the nadir point. The other is to direct an extremely powerful telescope to look directly downwards into a basin of mercury, and thereby to examine the motion of the image of the cross wires. But in neither of these ways shall we be able to trace motions exactly like those in the drawings, because actually the disturbances caused by the sun and moon are commingled, while the declinations are continually changing. The complexity of the resulting motions may be exemplified by reducing any one of the drawings to one-third part of the scale, so that it may represent the solar deflection, and by carrying its centre round any of the lunar curves for the same latitude, observing at the same time the distinction between the lunar and the solar day.

These speculations are interesting, not merely from the singular and unexpected beauty of the results, but also from their bearings on the higher branches of practical astronomy. They show us that the cross-level of a transit instrument is subject to a semi-daily oscillation, amounting at new and full moon to two forty-fifth parts of a second; and that the surface of the mercury in the collimation trough imitates, as far as it is able, the tidal motions of the sea.

Let us suppose that, by help of instruments capable of that degree of precision, we are endeavouring to determine the obliquity of the ecliptic true to the fourth decimal part of a second—we place our instruments in the observatory of St Petersburg, in Lat. 59° 56′ 30″, to observe the sun's meridian altitude at the

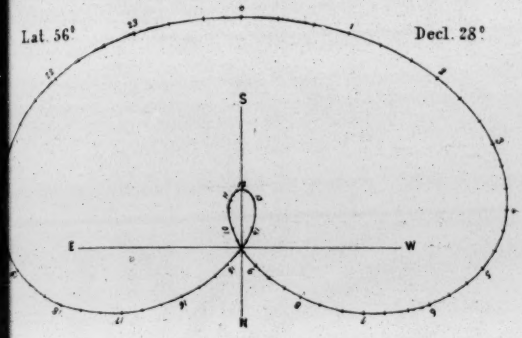
summer and the winter solstice. On account of the deflection, our summer observations give ·″0050 too little, and our winter ones ·″0012 also too little, thus giving the obliquity ·0019 too small. Simultaneous observations are carried on at Madras, in Lat. 13° 04′ 10″ north. At the summer solstice the sun is north of the zenith, and its altitude, counting from the south point of the horizon, is ·0019 too much, while the altitude at the winter solstice is too little by ·0047, thus giving the obliquity too great by ·″0033. Hence observations made at St Petersburg and at Madras must differ from each other to the extent of ·″0052, in determining this primary astronomical quantity.

Or if in the year 1857, we had sought to determine the greatest inclination of the moon's orbit, the greatest meridian altitude must have been ·0145, and the least ·0009, each too little, as seen from St Petersburg, giving the obliquity ·0068 too small; while, as seen from Madras, the upper passage would have appeared ·0082 too far north, and the lower ·0164 too far south, making an error in excess of ·0124, and thus the discrepancy of the two determinations would have amounted to ·″0192, or about the fiftieth part of a second.

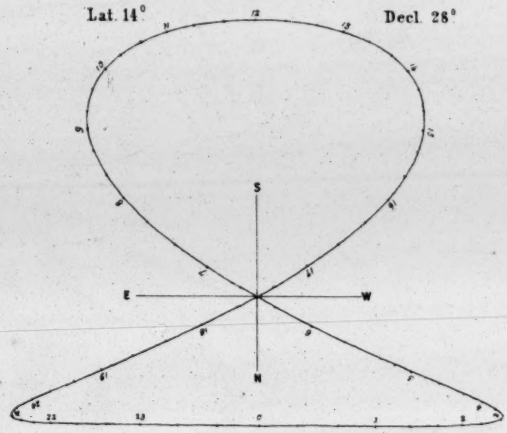
From these examples it appears, that we cannot carry our astronomical researches safely to the hundredth parts of seconds, without having taken into account the displacement of the nadir point. It is vain to seek to eliminate this source of error by the multiplicity of observations, because the influence varies periodically with the hour of the day, the moon's age, the position of the node, and even that of the apogee.



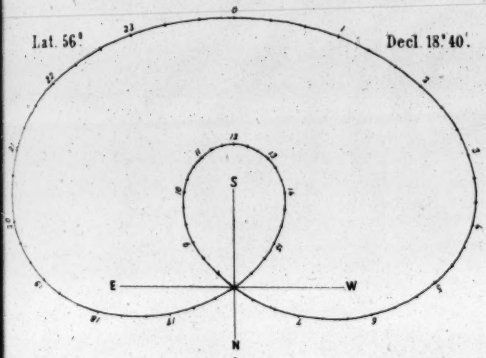
Lat. 56° Decl. 28°



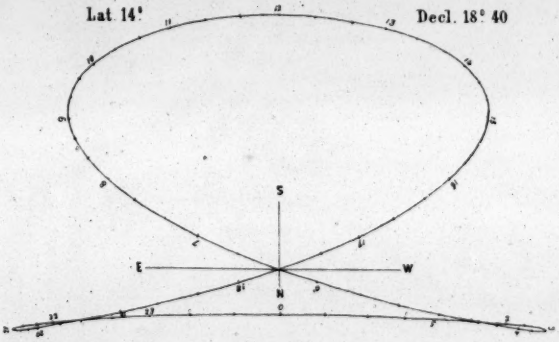
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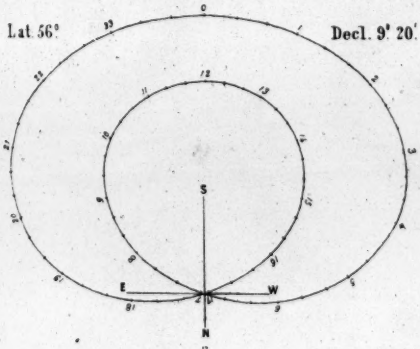
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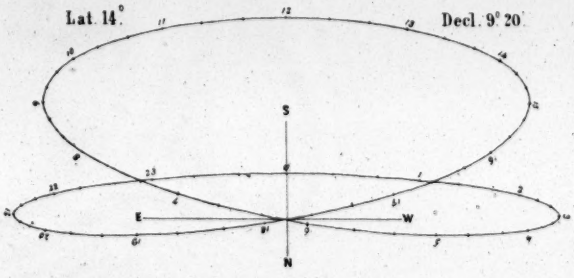
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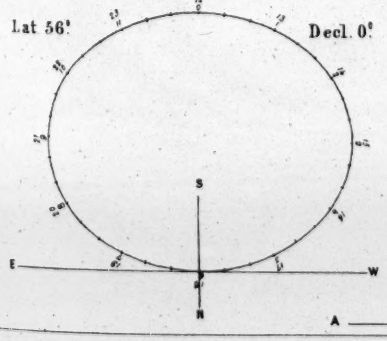
Lat. 56° Decl. 9° 20'.



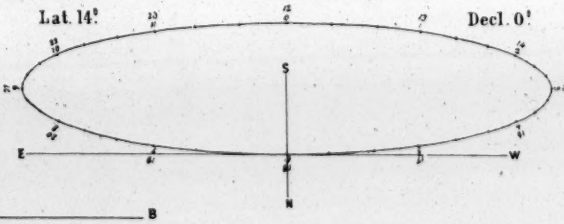
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Lat. 56° Decl. 0°



Lat. 14° Decl. 0°



A ——— B

W.H. Mc Farlane, Lith^r Edin^r

BRITISH
29 NO 62
MUSEUM

X.—*On the Existence of Acari between the Laminæ of Mica in Optical Contact.*

By Sir David Brewster, K.H., D.C.L., F.R.S. (Plate VI.)

(Read 28th April 1862.)

In searching for crystals of titanium between films of mica, I was surprised to observe something resembling the remains of insects. Upon submitting the specimen which contained them to a more careful examination, I found, in different parts of it, five completely formed Acari, all of which were much smaller than those found in cheese, brown sugar, and damp bran, specimens of which I obtained for the purpose of comparison.

The plate of mica, now on the table, which contains these five Acari, is about $5\frac{1}{4}$ inches long, 3 inches broad, and nearly the $\frac{1}{16}$th of an inch thick. The distances of the Acari from the edges of the plate are 1·6 inch, 1·13, 0·4, 1·0, and 0·5. The largest of the Acari is about the $\frac{1}{67}$th of an inch long, and the smallest the $\frac{1}{257}$th of an inch.

The films of mica between which the Acari lie were in optical contact—that is, no light was reflected from their contiguous surfaces. This fact does not prove that the surfaces had never been separated; for we may partially lift one film of mica from another, so that light is reflected from the separated surfaces, and yet they will return into optical contact. This is true even in a more rigid substance, such as glass, where a crack or fissure, produced in a thick plate by heat, will reflect light at the separated surfaces, and will afterwards entirely disappear, sometimes after the lapse of a few seconds, and sometimes after a much longer interval, according as the surfaces have been more or less separated.*

That these Acari were introduced between the laminæ from without, cannot be a matter of doubt; but in what manner, and in what stage of their existence, I do not presume to conjecture.

When I first discovered these Acari, they were much more perfect than they are in the accompanying drawings—executed for me by the Hon. Mrs Ward—as will appear from the rude and highly magnified sketches of the largest, made by myself. They had obviously suffered compression during their carriage by post, or perhaps from the object-glass of the microscope having been accidentally forced upon the plate in the process of searching for them with high powers.

If the specimen of mica is thought of sufficient interest to be preserved in the Museum of the Society, with the drawings that are not engraved, it may be proper to mention, that in trying to obtain a better view of the Acarus in the centre of the plate, Mrs Ward detached a film, which she afterwards replaced with cement. This fact is mentioned on her rough sketch of the plate, lest it might be supposed that the Acarus at that place had been artificially introduced.

* See Philosophical Transactions, 1816, pp. 73, 74.



Fig. 1.

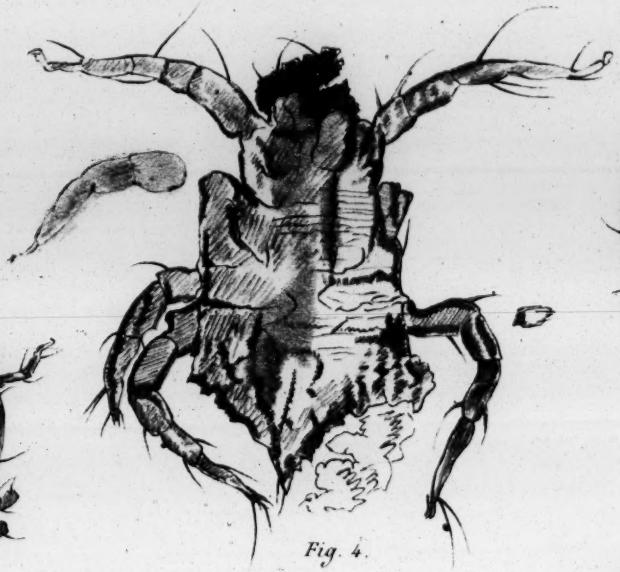


Fig. 4.



Fig. 2.



Fig. 3.

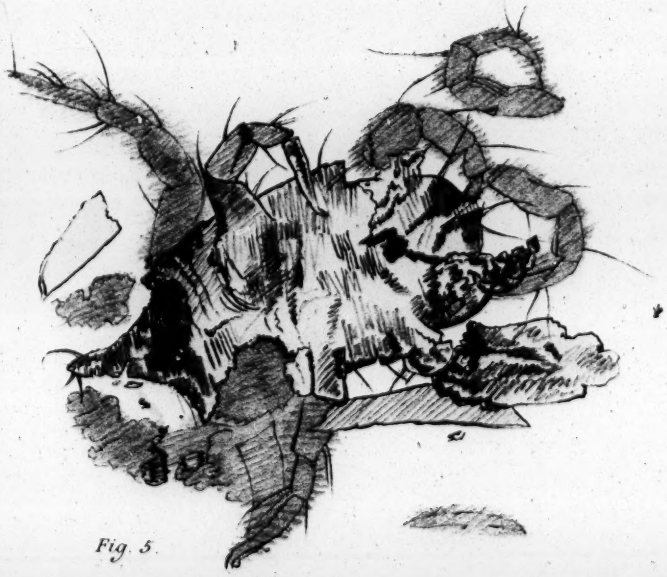


Fig. 5.



Fig. 6.

W. H. M.c Farlane, Lith.r Edin.r

BRITISH
29 NO 62
MUSEUM

XI.—*On certain Vegetable and Mineral Formations in Calcareous Spar.*

By Sir David Brewster, K.H., D.C.L., F.R.S. (Plate VII.)

(Read 28th April 1862.)

When occupied several years ago in studying the fluid and gaseous cavities in minerals, my attention was drawn to certain remarkable formations in perfectly transparent specimens of calcareous spar. These formations were of two kinds—the one apparently of vegetable origin, and the other obviously mineral.

1. The formations that appeared to be of a vegetable nature occur in specimens of calcareous spar from King's County, Ireland, and had been deposited on the faces of the primitive rhombohedron during its formation. The general aspect of the substance is that of a plant. Its colour is a dark green, which becomes lighter, and very brilliant, when the specimen is viewed by reflected light. These different effects, and the general structure of the substance, are finely shown in the accompanying drawings, executed for me by the Hon. Mrs Ward. In the figures magnified 60 diameters, a great number of minute spheres are seen scattered up and down among the stems and branches of what has every appearance of being a plant. They are more distinctly seen with a magnifying power of 100, and in the drawing, made with a power of 420, one of them has apparently burst and discharged its contents, as if it had been a seed-vessel, as shown in Plate VI., fig. 6. With the same high power the stems are distinctly tubular.

As the microscope could give me no further information respecting this interesting substance, I submitted specimens of the spar to my distinguished friend, Mr Andrews, Professor of Chemistry, and Vice-Principal, in Queen's College, Belfast, who favoured me with the following analysis of the extraneous substances:—

"The extraneous matter in the specimens is entirely destroyed by heating in a glass tube to incipient redness—a small quantity of water, having a strong empyreumatic odour, being at the same time given off. It dissolves in strong hydrochloric acid, without leaving any residue, but colours strongly the acid. Hence the extraneous substance appears to be of organic origin."

2. In another specimen of calcareous spar from India, the extraneous matter is arranged in fine parallel lines, which are generally, though not always, parallel to the faces of the rhombohedron. In order to determine its nature, I submitted the specimen to Professor Andrews, who obtained the following results:—

"I selected," he says, "two clear specimens, and placed them in hydrochloric

acid, so as to dissolve away a portion of the carbonate of lime. When the acid had reduced the crystals to thin laminæ, they were placed under a compound microscope, with an object-glass one inch in focal length, and viewed by reflected light. A number of yellow microscopic crystals were now seen scattered through the transparent plate of calcareous spar. They resembled iron pyrites, and were collected in great quantity in the position of two of the parallel bands. These crystals were not always cubes, but most frequently short fibrous masses, interposed in the cavities of the spar.

"So far all was pretty plain; but in dissolving away, by means of the hydrochloric acid, all the carbonate of lime in another portion of the specimen, instead of obtaining bright yellow microscopic cubes of iron pyrites, as I expected, the residual matter consisted of small fibrous masses, with a semi-metallic lustre and steel bronze colour. On analyzing them, I detected sulphur and iron, but no copper. I was for some time perplexed by the change of appearance in the crystals as seen enclosed in the matrix, and when left as a residue; and further, the microscopic aspect did not agree with the result of chemical analysis. At last it occurred to me that the crystals belonged to the prismatic pyrites (cock's-comb), and in crushing a fragment of that mineral, and placing it under the microscope, I found that its appearance exactly agreed with that of the substance under examination. The bronze colour of this variety of pyrites is quite superficial, and is removed by the action of hydrochloric acid. It is possible that the residual crystals may contain a little arsenic, but I had not enough of the material to pursue the investigation farther."

3. In another specimen of calcareous spar from India, the extraneous matter obviously consisted of minute crystals of copper pyrites. Professor ANDREWS found that they were composed of sulphur, iron, and copper.

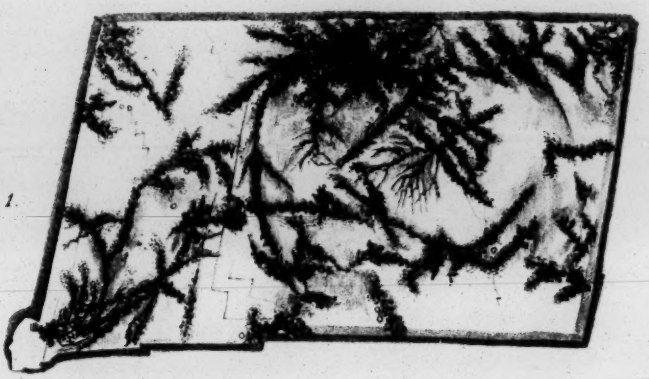


Fig. 1.



Fig. 2.

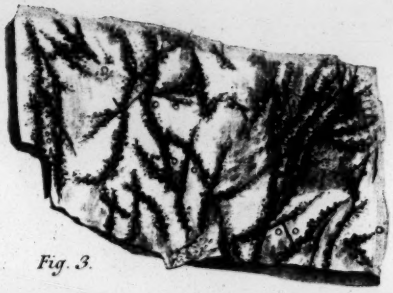


Fig. 3.

1 Inch magnified
60 Diameters.

Jas.s M.c Ward. del.t

W. H. M.c Farlane, Lith.r Edin.r

BRITISH
29 NO 62
MUSEUM

XII.—*Memoir of the Life and Writings of* ROBERT WHYTT, M.D., *Professor of Medicine in the University of Edinburgh from* 1747 *to* 1766. By WILLIAM SELLER, M.D., F.R.S.E., *Fellow of the Royal College of Physicians of Edinburgh.*

(Read 7th April 1862.)

It does not always happen that the memory of inquirers into nature, who have the merit or the fortune to strike first into a right path, is cherished as it deserves. This remark applies forcibly to the eminent person, whether regarded as a physiologist or as a physician, of whose life and labours a brief memoir is now laid before the Society. The name of ROBERT WHYTT was familiar to his contemporaries both at home and abroad. Increase of distance should hardly yet have dimmed its lustre. Yet, in proportion as the views which he initiated have expanded more and more in growing to maturity, the less and less is heard of their author. Biography—which never did WHYTT great justice—begins already to put him aside. A few particulars of his life, with a catalogue of his works, have hitherto been common in books of that description, principally in those of Germany and France. In some newer French biographies his name has dropped out. But of a late Edinburgh Biographical Dictionary, extending to not a few volumes, while restricted to the lives of eminent Scotsmen, it will hardly obtain credit that an early luminary of the rising University, conspicuous among the European leaders of medical science during a busy period of the eighteenth century, should, amidst a cloud of mediocrity, be there sought for in vain. ROBERT WHYTT, it may be said in defence, stands among the numerous followers of STAHL, who ascribed the phenomena of life to the operation of a rational principle; while history contents itself with recording the views of the master, without burdening its pages with the peculiarities of the disciple. But to place WHYTT among the followers of STAHL is to misinterpret the most essential points of his belief. Nor is this error an unlikely source of the scanty justice awarded to his merits. He, beyond doubt, embraced ideas subversive of STAHL's doctrines—ideas which have become the starting-point of views entering largely into the present aspect of physiological science.

In common, it is true, not only with the physiologists of the Stahlian school, but with those of all the preceding schools of physiology from HIPPOCRATES downwards, WHYTT traced animal movements to an anima or psyche; but he differed from STAHL, to borrow the description of HALLER, so widely, that he regarded such movements as being the immediate result of a stimulus, without any reason, intention, or consciousness on the part of the anima.

But if the sum of WHYTT's doctrine be that an impression conveyed by nerves to the central nervous organs excite involuntary animal movements, by a physiological necessity, without reason, intention, or consciousness, what is that doctrine but a comprehensive expression for the reflex action of the spinal cord and brain, if it be not also an approach to the still larger generalization which represents an organic body as consisting essentially of two layers, an outer for the reception of impressions, and an inner for the origination of movement? "*Quod autem est animal id motu cietur interiore et suo*," is the quotation from CICERO prefixed to WHYTT's earlier work on this subject—words which, as used by him, seem to embody the rudiment of this last idea.

But enough of preface.

ROBERT WHYTT was born at Edinburgh, September 6th, in the year 1714. His father was ROBERT WHYTT of Bennochy, a member of the Scottish Bar. He was a posthumous child, brought into the world six months after his father's death. He was still under seven years of age when he lost his mother. His mother's name was MURRAY. She was the daughter of ANTONY MURRAY of Woodend, in Perthshire. The WHYTTS of Bennochy were sprung from an old Fifeshire family, the WHYTTS of Mawe and Kilmaron. WHYTT's father was the grandson of the first possessor of Bennochy. The genealogy of the family is to be found in BURKE's "Landed Gentry," under the head "WHYTE MELVILLE," for a reason to be afterwards stated.

At an early age WHYTT was sent to the University of St Andrews, where, by his application to study, he appears to have distinguished himself among his fellows. Having obtained the degree of Master of Arts at the age of sixteen, he repaired to Edinburgh to engage in the study of medicine. Two years before, by the death of his elder brother, he had succeeded to the family estate. In the year 1730, when he commenced medicine, the Medical School of the University of Edinburgh was but recently founded. From the year 1720 the first MONRO had delivered regular courses of lectures on anatomy to still increasing audiences. Some years before WHYTT began his studies, the Town Council of Edinburgh (1726) had commissioned four Fellows of the Royal College of Physicians, namely, RUTHERFORD, SINCLAIR, PLUMMER, and INNES to teach medicine. Two years earlier, PORTERFIELD had been appointed by the same authority to the Professorship of the Institutes and the Practice of Medicine, but it is not certain that he ever delivered lectures. Soon after (1730), the Senatus Academicus recognised five professors as a Medical Faculty. About the same time, the Town Council instituted also a professorship of Midwifery in favour of Mr JOSEPH GIBSON, surgeon, known as the City Professor.

WHYTT devoted himself in particular to the study of anatomy under MONRO. He appears to have spent three or four years in Edinburgh, engaged in the acquisition of medical knowledge.

There are no incidents of his life recorded in the course of his studies; but among the medical manuscripts in the library of the Edinburgh College of Physicians, there is a book of notes taken by WHYTT, from the lectures of GEORGE YOUNG, who published, in the year 1758, a "Treatise on Opium." These lectures are partly clinical discourses on the cases which occurred in YOUNG's private practice; partly observations on diseases in general; partly discussions on the theory of medicine. The manuscript is of considerable extent, and sufficiently curious as a medical antiquity. It was purchased at the sale of WHYTT's books after his death, by Dr JOHN BOSWELL, who had been WHYTT's fellow-student under YOUNG, and came into the possession of the College along with a number of books presented by the family of the late Dr ABERCROMBIE.

Proceeding to London, probably in the year 1734, WHYTT became a pupil of CHESELDEN, while he visited the wards of the London hospitals. Thence, passing over to Paris, he occupied himself with the lectures and dissections of WINSLOW, and frequented the hospitals *La Charité* and the *Hôtel Dieu*. He next went to Leyden to hear BOERHAAVE, now advanced in years, and his namesake ALBINUS, still in the prime of life. It is well known that the name of ALBINUS was Latinised from "WHITE," probably in its German form "WEISS," as ALBINUS was of German extraction. Had ALBINUS been a native Dutchman, he might not only have been WHYTT's namesake, but of the same lineage; for, though the WHYTTS of Fife claim descent from the "LES BLANCS" of France, the presumption is far greater that, in common probably with the WHYTES of this island in general, they represent the WITTS of Friesland, one of whom, WITTA the son of WICTE (VETTA VICTI) the grandfather of HENGIST and HORSA, according to the probable conclusion of Professor SIMPSON in his very ingenious memoir entitled "The Catstane," lies buried beneath a gigantic monolith of greenstone on the banks of the Almond, but a few miles from Edinburgh. It is not recorded, however, that either the Professor or the pupil recognised in the other any sign of relationship.

It will come to be considered hereafter whether he then learned from ALBINUS a particular of the anatomy of the nerves for the assertion of which WHYTT's writings were for a long period singular. After six years employed in the study of medicine, he took the degree of Doctor of Medicine at Rheims, in April 1736. At that time, for what reason does not appear, Rheims was much resorted to for degrees in medicine. RUTHERFORD and PORTERFIELD, as well as WHYTT, had graduated at Rheims; while INNES had got his degree at Padua, SINCLAIR at Angers, and ALSTON at Glasgow. Of the forty-eight doctors of medicine who became Fellows of the Edinburgh College of Physicians between the beginning of the last century and the time when WHYTT was elected a Fellow, sixteen presented the Rheims degree; yet after that period down to the present, there have been only six, the latest being in the year 1753. The University of Rheims was suppressed at the first French revolution. In the autumn of the year 1737, the

University of St Andrews spontaneously conferred on WHYTT the same medical honour. In the end of that year he was admitted a licentiate of the Royal College of Physicians of Edinburgh, presenting both his degrees, and a year after he was elected to the Fellowship. He immediately commenced practice as a physician, and is said to have been very successful even at so early an age. Soon after he settled in practice, he married Miss Helen Robertson, who is described as sister to General Robertson, governor of New York. By this lady he had two children, both of whom died in infancy. The death of his wife followed, in the beginning of the year 1741. Two years after her death he married, in the year 1743, Miss Louisa Balfour, a daughter of Mr Balfour of Pilrig. Her brother afterwards became a professor in the University of Edinburgh.

By his second wife, WHYTT had fourteen children, only six of whom survived him. Mrs Whytt died in 1764, two years before her husband. His eldest son, Robert Whytt, came into his father's estate of Bennochy, but died young, without family, at Naples, and was succeeded in the estate by his next brother. He, on the death of General Robert Melville of Strathkinness, became heir to his entailed estates, and took the name of Melville in addition to Whytt. The orthography of the name is now slightly changed from the form which WHYTT retained down to the end of his life. His grandson is a well-known gentleman of the county of Fife, Mr Whyte Melville, living at Mount Melville, near St Andrews. His great-grandson, Mr J. G. Whyte Melville, younger of Bennochy and Strathkinness, is known to the public as the author of several popular works of fiction—"Digby Grand," "Guy Livingstone," "Good for Nothing."

In 1743, WHYTT published a paper in "The Edinburgh Medical Essays," "On the Virtues of Lime-Water in the Cure of Stone." For some years previously, public attention had been fixed on this subject by the grant of five thousand pounds by Parliament to Mrs STEPHENS, for her secret in the cure of this disease. The remedy was beyond doubt of some service in particular cases; but its disagreeable nature and great bulk gave rise to many attempts to throw out whatever in it was superfluous. Dr HARTLEY, the well-known author of the "Theory of Vibrations," who then practised at Bath, had already reduced the original farrago to two ounces and a half of soap, and seven scruples and a half of egg-shell powder, for the mean daily dose. Amidst the discussions on this subject, it occurred to WHYTT that lime-water might be sufficient to dissolve the stone. He accordingly prescribed lime-water as early as 1741, usually with the addition of soap, and obtained great success, while, in the paper just referred to, he gave an account of his cases. This paper attracted much attention. It was published with additions, separately, in 1752, and finally ran through several editions, in the later of which printed during his life, he expunged some of the ideas contained in the paper as originally given to the world. A French translation afterwards came out by ROUX, along with a translation of BUTTER'S

method of dissolving stone by injections. It is included in the German translation of WHYTT'S works, which was published at Leipsic soon after his death, in two parts, the one containing his practical, the other his theoretical writings.

WHYTT'S treatment of stone consisted in administering soap and lime-water to the extent of an ounce daily of Alicant soap, and three wine pints or more of lime-water. There is no reason to doubt that good results occasionally followed this treatment; but the benefit must have arisen from such effects as the correction of acidity and the sedative influence of the lime-water, rather than from any solvent power either in the soap or in the lime-water. In WHYTT'S time, no distinction had been established among the several kinds of urinary concretions. He however remarked, that some of the concretions which he met with were not equally affected out of the body by lime-water, so that, while he says such instances must be extraordinary, he limits his original conclusion by saying, that lime-water is, at least, a pretty universal solvent of calculous concretions. The concretions on which he generally experimented, must have come under the head of uric acid concretions; and in one of his experiments he hit on the most perfect solvent of that kind of concretion; for he put some grains of a calculus into a mixture of potass (carbonate of potassa) and quicklime, when he found the concretion to be entirely dissolved. He is even able, in the third edition of his work, to give an explanation of the result in the following terms:—'The quicklime takes the air (the carbonic acid) from the potass, and thus it is rendered much more caustic, and therefore fit completely to dissolve the concretion; but in the like proportion, the solvent is rendered too caustic for use in the living body."

It was not till ten years after WHYTT'S death that SCHEELE discovered the most frequent constituent of urinary concretions to be a concrete acid soluble in alkaline ley, namely, the uric acid, so as to lay the foundation of all that has been since ascertained with regard to the chemistry of such bodies. The reputation of other solvents has rendered the claims of lime-water obsolete in the present day, yet as these new remedies cannot act as solvents within the body any more than lime-water, it is possible that the beneficial effects of the latter may have been attended to of late less than they deserve. WHYTT had much correspondence respecting the cases of some persons of high rank affected with this disease; among these are Mr Walpole, afterwards Lord Walpole of Wolterton, the younger brother of Sir Robert Walpole, and Dr Newcome, Bishop of Llandaff, &c., whose cases, along with many others, are reported in his works.

Dr RUTTY* of Dublin, in 1741, had communicated a paper to the Royal Society of London, on the solvent power of soap lees and lime-water over urinary

* RUTTY on Joanna Stephens' Medicine for the Stone, 2d edit., London, 1745.

concretions out of the body; but he did not attempt to administer these remedies internally, till he learned WHYTT's method, as described in his first paper.

A great part of WHYTT's treatise is taken up with the investigation of the chemical character of lime-water, a subject, particularly at the time when the original paper was published, very little understood. In the third edition he takes the opportunity of stating how much light had very recently been thrown on the chemical relations of lime by the discoveries of Dr JOSEPH BLACK, then Professor of Chemistry in the University of Glasgow. WHYTT now understood the change produced on limestone and shells by fire; also why quicklime by exposure to air loses its acrimony, and becomes insoluble in water; again, why lime-water by the same exposure throws up an earthy crust and loses its solvent virtues.

In the "Edinburgh Essays, Physical and Literary," of date 1750, WHYTT published a paper on the various strength of different lime-waters. This paper, which is reprinted in the edition of his works edited by his son, was occasioned principally by objections raised by his colleague Dr ALSTON, Professor of Materia Medica, to some of his statements regarding lime-water. The controversy between the two professors is worthy of a short notice, were it for no other purpose than to add to the many examples, showing how insuperable, in things afterwards found to be most simple, the obstacles to discovery continue to be, so long as the true principle applicable to the case is unknown, as well as how much instruction is contained in the study of such impediments when once overcome. It seems to the present age almost incredible, that ingenious trains of experiments and long dissertations should have been necessary to settle such points as how much more soluble in water oyster-lime is than stone-lime, how much double lime-water exceeds single in strength, or why lime-water made in close vessels is more powerful than that made in open vessels. WHYTT and ALSTON, in common with the whole chemical world at that era, did not expect to dissolve any quantity of lime, however small, by throwing on it any quantity of water, however great. They did not believe lime-water to be a mere solution of lime, but held it to be a liquid formed by the impregnation of water with certain virtues derived from the lime. WHYTT regarded oyster or cockle shell lime-water as stronger than stone lime-water, resting this inference on the irrefragable proof, that the specific gravity of the former is greater than that of the latter. It were superfluous to trace out the particular sources of the discrepancy of opinion between the two professors. It does not appear, however, to have occurred to chemists of that period, till BLACK took up the inquiry, that what they called lime contained insoluble foreign matters, or that the water employed was not always of the same purity, or that it was necessary to attend to the variation of temperature in their several experiments. Yet this controversy between WHYTT and ALSTON proved the immediate cause of most important results. In the interesting account which

Dr BLACK gives of the circumstances attending his discovery of the nature of calcareous earth, magnesia, the alkalies, and their relation to fixed air,—the very foundation of modern chemistry,—he says, "I think it still worth your while to hear a history of the investigation." * * * "But I was peculiarly excited to it by the then recent discoveries of the power of lime-water to give relief in cases of the stone and gravel, in which it was supposed to act by dissolving those concretions, and expelling them out of the body. Dr WHYTT and Dr ALSTON, professors in this university, were then employed in a dispute on this subject. They both believed that it had efficacy; but Dr WHYTT imagined that he had discovered that the lime-water of oyster shell-lime had more power as a solvent than the lime-water of common stone lime. I therefore conceived hopes, that by trying a greater variety of the alkaline earths, some kinds might be found still more different by their qualities from the common kind, and perhaps yielding a lime-water still more powerful than that of oyster-shell lime." *

Thus the elaborate experiments of WHYTT and ALSTON on single and double lime-water, and on the comparative solubilities of oyster, cockle, egg-shell and limestone lime-water, however vain and fruitless they may seem on a cursory view, held a share in the origin of the great discoveries which, within a short period of their date, placed the science of chemistry on a wholly new and extended footing.

The original paper in "The Edinburgh Medical Essays," on the virtues of soap and lime-water, which grew into the treatise as it now appears in the collected edition of his works, was published, as already stated, in the year 1743. Up to this time the same professors had continued to teach in the medical department of the university.

Among WHYTT'S medical friends, Dr PRINGLE, afterwards Sir JOHN PRINGLE, stands pre-eminent. PRINGLE, after taking his degree at Leyden in 1730, had come to Edinburgh with the purpose of settling as a physician; nevertheless, in 1734, he accepted the office of Professor of Moral Philosophy in the university. In 1742 he commenced a new career, by becoming physician to the Commander-in-Chief of the British forces in Flanders. He retained his professorship till 1745, when he resigned. He settled a short time after in London, when WHYTT commenced a close correspondence with him on medical subjects, which was continued to the last year but one of WHYTT'S life. After WHYTT'S death, Sir JOHN PRINGLE assisted his son in the publication of the collected edition of his works.

In 1743 or 1744, Dr INNES, one of the four Professors of Medicine elected in 1726, fell into ill health. It appears that in the division of labour among these four professors, RUTHERFORD and INNES lectured on the practice of medicine, SINCLAIR on the theory of medicine, and PLUMMER on chemistry, or rather on

* BLACK: Lectures on the Elements of Chemistry, by ROBISON (1803), vol. ii. p. 53.

chemical pharmacy, while ALSTON, who had been long previously King's Botanist, taught after the year 1738, materia medica and botany. About this time SINCLAIR'S health also began to give way. The precise date of the death of Dr INNES does not appear. It was, however, during the confusion and interruption to the business of the university caused by the rebellion in 1745 and 1746. Neither is it certain at what time WHYTT first began to do duty in the university. The date, however, of the commission, appointing him Professor of the Theory of Medicine, is August 26, 1747. There is no doubt something confusing in this account; but the matter plainly stands thus: the death of INNES made a vacancy among the four original Professors of Medicine, which was supplied by the election of WHYTT; but INNES being a supernumerary professor of the Practice of Medicine, the real intention was, that the new professor should fill the place of SINCLAIR, who was no longer able for his duty, and whose successor he is sometimes said to have been. Accordingly it appears to have been stipulated on the appointment of WHYTT, that even if another vacancy should occur among the four original professors, there should be no new election. The minute of the Town-Council relative to the election bears, that Dr WHYTT, as substitute-professor, "had given universal content to all the gentlemen learned in that science." WHYTT ranks among the able professors placed in the university by the influence of Provost DRUMMOND. He was the most conspicuous citizen of Edinburgh at this era; and to him and the first MONRO the Medical School and the Infirmary, which go hand in hand, owe their early prosperity. In the long list of distinguished men appointed to chairs in the university under DRUMMOND'S auspices, there stand, besides the subject of this Memoir, COLIN MACLAURIN, MATTHEW STEWART, WILLIAM CULLEN, THOMAS YOUNG, MONRO *Secundus*, ADAM FERGUSON, Principal ROBERTSON, HUGH BLAIR.

If the chronicles of those times are to be trusted, another illustrious name must share in the merit of these appointments. Dr SOMERVILLE, in his "Life and Times," says, "I know it to be a fact, that Provost DRUMMOND, the most meritorious benefactor of the community over which he presided, did not find himself at liberty to promise any preferment at the disposal of the Town-Council of Edinburgh, without the previous consent of Lord MILTON, the delegate and political agent of ARCHIBALD DUKE of ARGYLE. To such an extreme was this scheme of universal patronage stretched, that it was always deemed prudent to obtain Lord MILTON'S good will, before making any application, even for places of the most inconsiderable emolument and importance. It was fortunate for the public, that in the enlightened scheme for filling the chairs in the university with the ablest candidates, the DUKE of ARGYLE concurred with Provost DRUMMOND." *

* SOMERVILLE, "My Life and Times," p. 380.

From the year 1744 to 1751, WHYTT had been engaged in the preparation of a work "On the Vital and other Involuntary Motions of Animals." In the latter year it was published. This is the work which fixed the attention of physiologists throughout Europe on its author. It was the misapprehension of certain expressions used by him in this work, which led to the current belief, not yet wholly obsolete, that his views largely corresponded with those of the followers of STAHL. Even HALLER inadvertently named him a semianimist, as if he had been the advocate of a part of the STHALIAN doctrines. Nothing can be more erroneous. WHYTT held nothing in common with the peculiar ideas of STAHL. A very few observations will suffice to prove how little there was of coincidence between his mode of thinking and that of STAHL.

The essential feature in the doctrines of STAHL is, that a rational, thinking, provident, conscious principle, originates and directs all the phenomena of living nature. It was a systematised extension of views which had never ceased to prevail from the earliest times. The ancients could discover no active agency in mere matter. Their poets peopled every moving scene with intelligent beings, Dryads, Hamadryads, Nymphs, and Demigods, to account for the visible effects. And when philosophers arose, they borrowed their source of force from man's own intelligence, and ascribed natural phenomena, inorganic and organic, to a soul; whence arose their soul of the world, their vegetative soul, their animal soul. The idea of the latter, in particular, was never lost sight of throughout the entire range of ancient physiology. The belief in the agency of the soul, as productive of the phenomena of animal life, is continued through the mystifications of the scholastic ages, through the paradoxes of PARACELSUS, through the profundities of DES CARTES, till it begins to be systematised in the vagaries of VAN HELMONT and STAHL. The activity relied on is an archeus, a psyche, an anima, a soul, a mind; that is, an agency framed on the conviction of the sufficiency of man's intelligence to determine new combinations of matter for the production of effects which he has premeditated.

There is still room in modern science for such a psyche. When the modern inquirer, not content with mere law, seeks the causes of organic phenomena, he cannot dispense with such an active force. As human intelligence is required to combine and regulate the natural forces which man avails himself of to produce his own works upon the earth, so with all the new found activity of matter derived from the interchanges of such forces as light, heat, aggregation, affinity, electricity, polarity, gravitation, a psyche is indispensable to direct the order and course of these forces in the development and working of organic bodies. Deduct the effects of all these natural forces in the development and working of organic bodies, and the residual force found to be necessary constitutes the psyche—a force just as essential in a "protococcus" as in the human frame. If it be otherwise sought for, it is nowhere else to be met with, except in the potentialities

existing in the reproductive cells derived from the first parent, or first parents of every species in the organic world.

Whatever else it may be, this psyche is a force; for, according to the received axiom in physiology, every act of life, every exercise of intelligence, every burst of passion, determines some change on the affinities in the textures concerned, while every change on such affinities is a source of force or motion.

WHYTT'S psyche was nothing different from that now referred to—such a psyche as is held essential by many modern physiologists, such a psyche as was upheld with much force of argument by the present Professor of Anatomy, in a discourse which he has not yet published, delivered to the Royal Medical Society several years ago.

WHYTT, then, it will be found, was no follower of STAHL; he was no animist or semianimist, any more than the major part of the medical world.

As already said, the "Essay on the Vital and other Involuntary Motions of Animals," was published in 1751. WHYTT professes to found his views on experiment and observation. He sets out with stating, that we have no reason to doubt that voluntary motion is produced by the immediate energy and agency of the mind, and that following the path pointed out by the ordinary simplicity of nature, he has endeavoured to show that all the spontaneous motions of animals are explicable upon the same principle, and owing to one general cause. He remarks that STAHL, "by extending the influence of the soul, as a rational agent, over the body, a great deal too far, has been the occasion why, for many years, it has been considered rather as a subject of ridicule than deserving a serious answer." He cites a passage from LEIBNITZ, in which that philosopher supposes, "that the natural motions may be owing to some impressions made on the mind, although we are no ways conscious of them." "But there is no need," WHYTT says, "of understanding the nature of the soul, or the way in which it acts upon the body, in order to know that the vital motions are owing to it; it is sufficient if we know from experience that it feels, is endued with sensation, and has a power of moving the body." "It is no sufficient objection that we are unconscious of the interposition of the mind in the vital and other involuntary movements; for some, he continues, of the voluntary motions, are performed, while we are insensible of the power of the will being exerted in their production." * * * * "Some, indeed, have gone so far," he says, "as to deny that even voluntary motions are owing to the mind as their proper cause, and have thought the direction of the voluntary muscles, in order to perform the various motions of the body, to be an office which its faculties are not equal to. But if these motions be not owing to the mind, from what cause, external or internal, do they proceed? They cannot be owing to the body alone; and it is in vain to attribute them to any law which it may be pretended the Deity has established, since a law can produce no effect of itself; and without some agent to execute, it is only

a mere name or empty sound; they must therefore be ascribed either to the immediate agency of the SUPREME BEING, or to that of some general inferior NATURE which HE has constituted for this purpose, or to the energy of a particular active principle united with the body. The first two suppositions are indeed possible, but not probable, as is the last; whence it may be inferred, that not only the voluntary motions, of which we are immediately conscious, but those also which we do not advert to, proceed from that sentient and intelligent principle with which the Creator has animated our bodies, whose powers and operations, it must be owned, are in many instances as much above our knowledge, as is the nature of its union with the body, or the manner of their reciprocal action upon each other."*

These few passages will afford some insight into the manner of WHYTT'S reasoning, and at the same time show the kind of language which he habitually employs.

In considering more in detail the system developed in this work, it is essential to take note of the two distinct subjects embraced in the title; namely, the vital motions of animals, and the involuntary motions of animals, such as, in the language of physiology, are not vital. It has so happened, that the views entertained by WHYTT, respecting vital motions, namely, the contraction of the heart, the contraction of the arteries, and, generally, the action of the organs concerned in the maintenance of the living system, have not, in their full extent, been very largely adopted by modern physiologists; that is, to the extent of regarding the contractility of the moving fibre, as being dependent on an influence derived from the nervous system. Recent physiology nevertheless fully admits that all such organs are retained in harmony with each other, and with the whole system, by means of the nerves distributed to them. Whence it follows, that much even of what WHYTT taught under the head of vital motions, in a certain measure, represents modern ideas. On the other hand, the views enunciated by WHYTT respecting such involuntary motions of animals as are not vital, are found to be not only the foundation of the present ideas as to these movements, but to represent the largest generalization which has been reached as to the general nature of activity in the organic world.

WHYTT'S great claim to a permanent fame, therefore, is that he was the first to strike into a right path of investigation, in respect to the relation, in organic nature, between external impressions and the movements determined from within the organism. Between this generalization and the doctrine of WHYTT, in regard to the non-vital involuntary movements of animals, it is easy to trace the connection step by step. WHYTT, throwing aside the rational soul of the Stahlians as a cause of non-vital involuntary motion, ascribed such movements to the effect

* Works, pp. 140–160.

of a stimulus acting on an unconscious sentient principle. He does not say that the animal is necessarily unconscious of the stimulus; neither does he say that the animal is unconscious of the muscular contraction which constitutes the movement; what he enforced is, that there is no consciousness of anything interposed between the consciousness of the impression, and the consciousness of the muscular contraction excited. For example, if a person sitting with his naked foot on the fender, sees a tea-kettle about to boil over, he withdraws his leg; if on the other hand, some drops of boiling water fall on his foot without warning, his leg is snatched back in spite of himself. In both cases there is the consciousness of an impression preceding the withdrawal of the limb; but between the sight of the kettle about to boil over, and the consciousness of the muscular contraction by which the leg is moved backwards, there is the consciousness of a voluntary act, the consciousness of the act of mind termed a *volition*. In the second supposition, on the contrary, between the consciousness of the sensation caused by the hot drops of water, and the consciousness of the muscular contractions concerned in moving back the leg, there is nothing directly discovered to intervene. Here WHYTT, not having learned to lay aside the philosophy of causes, and to content himself with the expression of a law, infers that the movement is caused by an unconscious act of a sentient principle. Had WHYTT in such a case omitted to assign a cause, he would not have satisfied those to whom he addressed himself. Men were not then accustomed to be put off with a law of succession between two phenomena in lieu of a cause. Doubtless they were then too often treated to a supposition instead of a reality, when they asked for a cause; yet not oftener than in the present day a law is made to pass for an explanation, when it is destitute of any such significance, being an individual case in a species of facts, instead of being an example of a species known to belong to a genus of facts. Reflex action, in all its vast extent, is but a species, which not being yet referrible to a genus, receives no explanation,—it is a fact that may be multiplied to the utmost extent, without exhibiting anything of a generic character. Gravitation itself would form but a species of facts, did it not include the heavens, as well as the earth.

To resume, WHYTT moreover explains, that he could not dispense with this sentient principle, because he saw no other mode of connecting the impression from without with the more or less distant sources (that is, the origins of the efferent nervous filaments) of the influence from within, by which a complex outward movement was to be effected. Had the idea been developed in his day, of the white fibres of the nervous system being merely internuncial, then there would have been no need of such a principle.

He represents, it is true, this principle as a part of the soul; but doubtless he would have been content had he believed in the purely internuncial office of the white fibres to reduce it at least to its modern rank of a *vis nervosa*, which indeed

is hardly less of an assumption than his sentient principle. How little he was dogmatic on this point, is proved by the following paragraph:—"If any one should yet contend that the sentient principle governing the vital motions is yet different from the rational soul, I shall not further dispute the matter with him; since whatever is advanced in the present essay upon the subject of the involuntary motions of animals, will hold equally true, whether the sentient and rational soul be supposed distinct or otherwise."*

Though WHYTT made no pretension to advance the knowledge of his day as to the mere anatomy of the nervous system, the merit may be claimed for him of having first published a view which was then only a probable assumption, but which is now regarded as fully established. The point referred to constitutes one of the most prominent features in the recent descriptions of that system, namely, the complete isolation of the minute filaments entering into the nerves; in other words, the belief that their component filaments are unbranched, and quite distinct one from the other, from their origin to their termination. Though some confusion of ideas exists on this subject, which after WHYTT's time was never forgotten, it will be easy to bring proofs, as will be seen hereafter, that he was the chief authority thereon for the long period of seventy years.

But it is impossible not to perceive that the belief in the complete isolation of the filaments of the nerves must have been the direct step to the idea, whether that idea shall or shall not require after modification, of the white fibres of the nerves and medullary substance being merely conductors of influences generated elsewhere, and therefore to the correlative notion of the grey matter being the seat of force. Discovery still proceeded in the path indicated by WHYTT, when the nervous system in animals low in the scale of being was shown to consist of a detached series of nervous knots connected by commissures, and communicating with the external surfaces by nervous filaments. And even when animals were remarked destitute, like vegetables, of nerves, but still performing movements analogous to those determined through nerves, so as to render it necessary to omit all reference to nerves, by saying that one animal surface receives impressions and another originates movements, there is still merely an extension of WHYTT's proposition, however unwilling he might have been, if still in the world, to bow to its truth. What is here claimed for WHYTT, is merely the initiation of one or two principal steps in the way to this generalisation, without in the least derogating from the unquestionable merit of the subsequent labours, particularly of UNZER, PROCHASKA, and MARSHALL HALL.

Besides these more general observations, a few words in detail may be added for the full understanding of the views which WHYTT adopts. He does not attempt to systematise the animal movements beyond the common division into

* Works, p. 150.

voluntary, involuntary, and mixed. He regards the purely involuntary and mixed sort, in reference to this principle, as falling under the one head of spontaneous movements. The purely involuntary movements to which he chiefly refers are the vital movements before enumerated, such as the contractions of the heart, those of the blood-vessels, and the like; but as it is designed to speak of these apart, all reference to them is for the present avoided.

Many of the spontaneous movements which he cites as examples of his principle are of the mixed kind, or at times under the control of the will. A few of them, such as sneezing, cough, hiccup, and vomiting, may be pronounced absolutely involuntary. Further, some of them contrast with others as being very simple in their character, such as the closing of the pupil under a strong light, the shutting of the eyelids when the eye is threatened, the adjustment of the membranes of the internal ear by the small muscles within the tympanum to the variations of sound. These obtain the name of simple, because, while the afferent nervous filaments by which the outward impression is conveyed to the nervous centre must all terminate in close proximity to each other, the efferent filaments by which the motor-influence is transmitted to the muscular fibres concerned, can hardly originate at any considerable distance from the former or from each other. On the contrary, such spontaneous movements as respiration and its many modifications, of which sneezing, cough, hiccup, are instances, full vomiting, deglutition, the evacuation of the rectum, and of the bladder, while these movements are still exempt from the control of the will, and a large class of cases resembling voluntary acts in which the muscular effort is certainly involuntary, or at least without intervening reflection, as in the case of hot water falling on the foot, are all truly named complex acts; because, though the afferent filaments conveying the outward impression in these examples may spread over but a small area of the sensory organs, the efferent filaments carrying outwards the motor influence being necessarily numerous, would seem to originate from points distant from the former, and distant from each other in the central organs.

The principle on which WHYTT explains all such cases is, that the outward impression (outward as respects the nervous centre) made on the peripheral extremity of a nervous filament being conveyed to the brain, determines the transmission of motor influence by another nervous filament reaching the muscular fibres which are to be called into activity; and that this effect takes place, however distant the origin of the second filament, producing the motion, may seem to be from the termination of the first filament, producing the necessary affection of the nervous centre, that is, according to him, an unconscious operation of mind, or of a sentient animal principle.

The discovery of numerous before unnoticed relations between the several parts of the nervous system, much as it has improved the knowledge of the purposes served by the peculiar conformation of the nervous organism, has not ma-

terially bettered the understanding of the principles of activity therein operative. The same forms of expression are still applicable to the principle recognised by WHYTT, and to that acknowledged in the systems of the present day; namely, the continual application of such and such a stimulus succeeded by the appointed effect of each stimulus according to its nature, and the circumstances attending its operation.

Numerous authors are cited from ARISTOTLE to BOYLE, GLISSON, REDI, WOODWARD, and KAAU BOERHAAVE, who have vaguely spoken of motions in animals after decapitation, as if they had anticipated WHYTT in his peculiar views.

Of these, GLISSON, HARVEY'S contemporary, who, as the author of the idea of irritability independently of the nerves, holds a high rank among physiologists, ascribes the movements in decapitated animals to what he terms perception of impressions without sensation. It is possible that the idea in GLISSON'S mind justly represented the modern reflex action; but his mode of expression is too vague to have had much influence on the progress of this part of physiology. WHYTT recounts the experiments of REDI and KAAU, such as removing the brain and cutting off the heads of tortoises, without destroying life even for many months; and suddenly cutting off the head of a cock in rapid motion, after which he continued to run for several yards. Such experiments led WHYTT to make the following observation, which brings his ideas still more close to the modern notion of the reflex action of the spinal cord,—"the spinal marrow does not seem altogether derived from the brain and cerebellum, but probably prepares a fluid itself, whence it is enabled to keep up the vital and other motions for several months in a tortoise after the head is cut off."

To discover the extent of WHYTT'S influence on the subsequent progress of the physiology of the nervous system, some notice must be taken of the labours of a few of his immediate and more recent successors, particularly UNZER, PROCHASKA, and MARSHALL HALL.

UNZER or UNTZER of Halle, though younger than WHYTT, began to publish before him. But his "Principles of Physiology," the only work in which he touches closely on WHYTT'S subject, was not published till some years after the death of the latter. For an excellent translation of this book, published in 1851, English physiologists are indebted to Dr LAYCOCK. UNZER was well acquainted with WHYTT'S writings. Speaking of what he terms nerve-forces, he says,—"Modern physiologists, whose names Europe knows and honours, HALLER, ZIMMERMAN, WHYTT, and OEDER, have rendered much service to this department of physiology, by contributing materials thereto."

It has been imagined that UNZER'S views are far in advance of WHYTT'S This idea rests on a misunderstanding of what UNZER taught. UNZER'S book is indeed full of ability, and is even at this day of surpassing interest to the physiologist, but it abounds in language peculiar to the author to so great an extent,

that before the light which recent discoveries have thrown on the subject it must have been difficult to decipher. Whence it is probable that the influence of UNZER'S writings over the progress of nerve-knowledge has often been overrated. What may be described as his general view of "vital and other involuntary motions," does not differ essentially from WHYTT'S,—that is, he regards them as originating in an external impression, conveyed by nerves to the brain and mind, whence a motor influence is transmitted by nerves to the organ to be called into motion. In one respect, under this head, he goes beyond WHYTT, namely, in the full manner in which he treats of instincts, "which," he says, "nature has so placed under the control of external impressions, and so arranged the natural functions of the organs, that animals cannot prevent their manifestation." But in what has been mistaken for a nearer approach to the modern "reflex action" in UNZER'S views, there is really a retrograde movement. The word "reflex" is employed by him in a sense altogether different from its acceptation at present. When he says that sensory impressions are reflected into motor force, what he means is, that reflexion takes place through communications between nervous filaments, altogether independently of the brain or spinal cord. And while he speaks of ganglia being sometimes concerned in this reflexion, he does not regard those as subordinate nervous centres, but merely as mechanical obstacles, along with others which he names, by which the ascent of an impression to the brain is impeded.* Whence it follows that this kind of reflex action is merely a revival of the old view of WILLIS as to sympathetic acts being the effect of communications between nerves in their course,—an idea rejected by WHYTT, and disproved by MONRO.

PROCHASKA, who appears next after UNZER to have taken up the views initiated by WHYTT, belongs to the last quarter of the eighteenth, and the first twenty years of the nineteenth century. He was Professor of Anatomy and Diseases of the Eye, first at Prague for twelve years, and then at Vienna. His Latin work, "De Functionibus Systematis Nervosi," was first published in 1784. He shows himself familiar with the views of WHYTT, whose works he repeatedly quotes. He is also well acquainted with UNZER'S works. It is undeniable that PROCHASKA made a considerable advance on the views of WHYTT. And yet a few quotations will prove how little these two physiologists differ in principle. "Thus," PROCHASKA says, "although a nerve be necessary to sensation and motion, it does not excite motion or feel alone, but feels by means of the brain, which, when an impression made on a nerve is brought to it, communicates the impression to the mind; and the nerve produces motion by means of a muscle, when the nerve descends to the muscle and excites it to movement."† Such a statement might

* Unzer, "Principles of Physiology," translated by T. LAYCOCK, M.D., pp. 223, 224.

† Prochaska, "Dissertation on the Functions of the Nervous System," translated by T. LAYCOCK, M.D., pp. 407, 408.

readily have been made by WHYTT. The next quotation shows an advance upon WHYTT'S ideas.

"It certainly does not appear that the whole of the cerebrum and cerebellum enters into the constitution of the sensorium commune, for the former portions of the nervous system seem rather to be the instruments that the soul directly uses for performing its own actions, termed animal; but the sensorium commune, properly so called, seems not improbably to extend through the medulla oblongata, the crura of the cerebrum and cerebellum, also part of the thalami optici, and the whole of the medulla spinalis; in a word, it is co-extensive with the origin of the nerves. * * * The reflexion of sensorial into motor impressions, which takes place in the sensorium commune, is not performed according to mere physical laws, where the angle of reflexion is equal to the angle of incidence, and where the reaction is equal to the action; but that reflexion follows according to certain laws, writ, as it were by nature, on the medullary pulp of the sensorium, which laws we are able to know from their effects only, and nowise to find out by our reason. * * * Since the principal function of the sensorium thus consists in the reflexion of sensorial impressions into motor, it is to be noted that this reflexion may take place either with consciousness or without consciousness."

Thus reflex action in the hands of PROCHASKA amounts to this, that it has its seat in the commune sensorium, or that part of the nervous centre which, excluding the cerebrum and cerebellum, is sometimes named the cranio-spinal axis. He differs from WHYTT, therefore, in excluding the cerebrum and cerebellum from any share in the function; and while his language is not the same as WHYTT'S, because he adopts the term law to express its character, which WHYTT had refused to use, he is essentially of the same way of thinking with WHYTT, inasmuch as he says the law is written on the medullary pulp of the sensorium, which is merely another mode of stating that there is something behind that pulp whereby the effect is produced.

Before leaving PROCHASKA, however, reference must be made again to the share WHYTT had in fostering and insisting on the assumption of the complete isolation of the minute fibrilæ composing the nerves. PROCHASKA, who was well versed in the literature of the nervous system, refers to the isolation of the fibrils of the nerves as a conjecture of WHYTT'S, while he rejects it as an improbability. Nevertheless UNZER distinctly says, that no confusion of impressions brought by separate channels to the brain occurs, because the nerve-fibrils are isolated. Here he seems manifestly to contradict himself in a passage before referred to, in which he says, that "the impression is transmitted along the nerve upwards towards the brain; but ere it reaches there, it is turned from its course, and so reflected downwards, that it excites (like an internal impression) the nerve of the other remote parts or the nerve twigs, or efferent nerve-fibrils of the part receiv-

ing the impression." Here one would suppose that the reflexion takes place without the brain being reached, because of a connection between the afferent and the efferent nerve-fibrils concerned; and if that be not the case, it must be owing to something like "induced contraction" for example, to the *vis nervosa* of the afferent fibril being communicated to the efferent fibril. Be this as it may, it does not appear to have been observed by most authors, any more than by PROCHASKA, that UNZER maintained the idea of the nerve-fibrils being isolated from their origin to their termination.

WHYTT's statement on this point is clear and distinct. It was not, however, published sooner than the year 1764. It is not impossible that he learned this supposition from ALBINUS, when he studied at Leyden; but it is not contained in any of the published works of ALBINUS. Nor could it have been, as has been asserted, an early received idea of the Boerhaavean School; for it is not referred to in BOERHAAVE'S Institutes,—not even in the form of a question,—amidst the host of questions, where it could not but have had a place, if it had ever stood among his speculations. In the manuscript notes of GEORGE YOUNG'S lectures, taken by WHYTT before he went to Leyden, the nervous fibrils are spoken of as communicating "to appearance," as if the question of their isolation had been already raised at Edinburgh. Sir WILLIAM HAMILTON, the late Professor of Logic, in one of his elaborate notes to his edition of REID'S Works, in which he reproaches modern physiologists with their ignorance of what had been done by their early predecessors, with respect to the minute anatomy of the nervous system, ascribes the origin and elucidation of the idea of the complete isolation of the fibrils of the nerves to ALBINUS, on the authority of two manuscript copies of his lectures, one taken in the year 1741, the other some years later. The ridicule which Sir WILLIAM throws on MÜLLER, for the belief that he, in 1830, had first established the fact of the primitive fibres of the cerebro-spinal nerves being isolated and distinct, from their origin to their termination, would have been hardly deserved, had there been no record of any previous belief in that fact but the two manuscript copies of the lectures of ALBINUS, of which Sir WILLIAM enjoyed the monopoly. It could hardly be that MÜLLER had never heard of the idea till that time. This is not what he means, but that he had established as a fact, what was before only a probable conjecture, of which WHYTT was the reputed author. Of WHYTT, Sir WILLIAM makes no mention, though he plainly took his quotation of MÜLLER'S physiology from the translation by Dr BALY, in which, at the foot of the same page, he must have seen the note by that much lamented physician (whose recent terrible death still makes the flesh creep), to the effect that WHYTT taught the same view seventy years before.

A minute detail of the progress of the subject would be unsuitable to this occasion. It may suffice to indicate the names of those who have taken the largest share in its advancement,—Sir GILBERT BLANE, LEGALLOIS, HERBERT MAYO,

FLOURENS, MÜLLER, W. P. ALISON, MARSHALL HALL. MARSHALL HALL'S conclusions, though not all implicitly received, are undoubtedly, for the most part, in advance of all previous views. Dr CARPENTER, while he admits the reflex action of the cranio-spinal axis, in the sense taught by MARSHALL HALL, insists on its connection, for the maintenance of many complex acts, with sensation as existing in the sensory ganglia or nervous parts whence the nerves of special sense originate. Under the name "ideo-motor," he describes such acts as those of the somnambulist as being the result of a reflex action of the cerebrum itself, while he applies the expression "unconscious cerebration," to a supposed reflex action of the cerebrum which may proceed without the knowledge of the individual, so as to evolve intellectual products in the shape of new views of subjects previously before the mind. On this latter point, Dr LAYCOCK entertains similar opinions, and appears successfully to have vindicated to himself the claim of having been the first who conceived the idea of a reflex action of the cerebrum. This idea is highly ingenious, and plainly an extension of that developed by WHYTT, but it requires a larger consideration than the limits of this memoir permit.

The starting point in the further investigation of this subject is obviously the following acknowledged fact, the close correspondence between which and WHYTT'S view is indisputable—an impression made on the peripheral extremity of a nervous thread is conveyed to a segment of the cranio-spinal axis, and is imparted to one of the cells in its vesicular substance, whence an influence is transmitted horizontally, by means of a white internuncial thread, to a cell of the like substance, where motor power being generated is made to act on a contractile fibre, by a nervous thread extended between the motor-cell and that fibre.

This faculty of motion may be described as a primary power of the living frame; that is, as conferred upon it by its original constitution. It seems not improbable that peripheral impressions are, by the same original constitution of the frame adjusted to the production of even very complex motions, without any controlling influence from other parts of the nervous centre. In one of MAYO'S experiments, when the sole of the foot was pricked, after division of the spinal cord in the back, the foot was suddenly retracted "with the same gesture as it would have been during life." The extent to which the skeleton muscles must be called into activity in automatic acts, produced by peripheral impressions, long before volition begins to be exercised, cannot but be very great. Hence, probably, the early extensive exercise of this primary reflex action, in the infant, may have very momentous effects on the development of the whole function of locomotion in the living frame.

Besides peripheral impressions, there is another extensive set of causes continually in force from the earliest infancy to the production of automatic acts in the skeleton muscles; namely, emotions, passions, and the various instinctive affections which originate in mental operations. This set of causes, doubtless,

takes effect on appointed motor-cells in the cerebro-spinal axis, through vertical internuncial threads in that axis. Whence arises a question for investigation, whether each contractile fibre has a cell in the cerebro-spinal axis appropriated to itself, and whether the same cell determines the contraction of that fibre in the case in which the exciting influence is conveyed by a horizontal internuncial thread, as well as when conveyed by a vertical internuncial thread. To this may be joined another question; namely, whether, when the same contractile fibre is acted on by volition, the effect be produced through the same cell and the same vertical internuncial thread.

Volition is plainly of the same character when exercised over thoughts as when exercised over impulses to muscular contraction. It has no direct power over either; but by frequent exercise it can command both with a great degree of certainty. If a thought has been once present to the mind, and has received a certain amount of attention, it belongs to the memory, and may usually be recalled when required. So the consciousness of the contraction of a muscular fibre, by attention, becomes an object of memory. When it is recalled, the effect of the act by which it was before produced is recalled also. In this manner volition plainly extends its power over the muscular frame in such acquired efforts as skating, tight-rope dancing, and the exhibitions of the slack-wire. But it is not improbable that the memory of the consciousness attendant on the automatic contraction of a muscular fibre in the early period of life, is the source whence volition derives its first power over the muscular system. The consciousness attendant on the contraction of a set of muscular fibres must be the same, whether the act be automatic or the effect of volition. Whence the remembrance of the consciousness must be the same, from whichever source derived. But if the remembrance of an act determined by volition be the condition on which it can be repeated, the remembrance of an automatic act should be sufficient to produce that act, even though it had never before occurred under volition. Volition is commonly represented as conative, or attended with a particular effort; but if it simply exert an influence on the motor cells of the cranio-spinal axis connected with the motor threads spreading to the muscular fibres concerned, there is no special effort necessary, beyond that which these cells would have exerted had the movement been determined by horizontal internuncial threads arising from cells acted on by afferent nervous threads from a cutaneous or mucous surface. If there be anything entitled to the name of effort, it must be the same in a simple reflex act as in a voluntary act.

Hitherto but a part of the doctrine taught by WHYTT in the work under consideration has been exhibited. He extended his principle to account for the ordinary action of the heart, blood-vessels, the organs of secretion, and generally to all the organs of the living body, concerned in what is called its vegetative life. This department of the subject he worked out with much pains, and endeavoured

to illustrate by numerous experiments. It was in this part of his inquiry that he fell into controversy with HALLER. This controversy led to the publication of three treatises, two, namely, in a separate form, and one in the "Edinburgh Physical and Literary Essays." These treatises are named respectively, "An Inquiry into the Causes which Promote the Circulation of the Blood in the very small Vessels of Animals;" "Observations on the Sensibility and Irritability of the Parts of Men and other Animals;" "An Account of some Experiments made with Opium on Living and Dying Animals." All these bear on the question at issue between him and HALLER, principally as to the dependence or non-dependence of irritability, now called contractility, in muscular fibre, on the presence of nerves. It was already remarked, that recent physiology for the most part rejects WHYTT's view, in so far as it attributes to nerves the vital properties of the solids concerned in such vegetative acts, while it fully admits the influence exercised by the nervous system over their course and order. If this distinction be borne in mind, the precise bearing of the following passage from one of our most recent and most distinguished cultivators of the physiology of the nervous system on WHYTT's doctrine, will be fully understood:—"Before treating," says Mr BROWN-SEQUARD "of the reflex changes in nutrition, which are by far more frequent and more important to be well investigated than the reflex secretions, I must remark that the reflex character of facts more or less similar to those I have to mention has been known for a long while; and that the modern theory is not far in advance of that given in this respect by ROBERT WHYTT in the last century. In one of his important works he has shown that the normal and morbid sympathies either for movements, nutrition, or secretion, are reflex phenomena. Still more, he has shown that the share of blood-vessels is very great in many of these phenomena." * In all such cases, however, it is not to be admitted that the actual properties of the molecules concerned are determined by nerves, but only the course and order of molecular action.

It will be enough to show, in a few words, the nature of WHYTT's controversy with HALLER as to the contractility of the muscular fibre. What he insisted on, in opposition to HALLER, is, that the contractility of muscular fibre depends on an influence derived from the nerves,—that is to say, he gave to the nervous power the double office of imparting to muscular fibre the susceptibility of contracting on the condition of the application of a stimulus, and, as a stimulus, of exciting this susceptibility into actual contraction. The influence conveyed by a nerve to a muscular fibre is manifestly one of the causes by which its contraction is excited. But it does not at all follow that a similar influence conveyed by the nerve should impart to the muscular fibre its susceptibility of contracting. A muscular fibre contracts under the application of other kinds of stimulation,

* Lectures, "Lancet," 1858, vol. ii. p. 468.

even after its communication by nerves with the nervous centre is destroyed. The onus of proof, therefore, that nervous influence is necessary to maintain the susceptibility of contracting, obviously lies on those who take the affirmative side of the question. Contractility is present to a certain measure in the vegetable kingdom, and in animals destitute of nervous system, whence it may be concluded that the organic structure, independently of nerves, is susceptible of the endowment of contractility. If it be said that mere structure cannot assume the vital property of contractility, the answer is, that the difficulty is only thrown farther back by imposing the communicating power on the nervous organism. Such an organ as the heart may be supposed to possess contractility, independently of its nerves, by its original structure; but it is not necessary to admit that nothing but physical force is concerned in its production. All the physical forces known to operate in living nature may have been concerned in its development; but if from the whole amount of their known efficiency an unsatisfactory solution is obtained, then the residue of force or of direction requisite must be sought for in the potentialities of the reproductive cells derived from the first parents of the species. On this principle only can the development and properties of organic structure be placed. Nor is this view to be rejected as out of the line of physiological evidence. The case is quite parallel to that of the main-spring of a watch. The motive property of such a spring depends on its artificial chemical constitution, and the manipulations necessary to impart to it proper temper; all these are the effects of physical forces, directed nevertheless by human intelligence. Thus, even in the effects of a watch-spring, there is present an effect of the *Divinæ particula auræ*, which circumstance renders the case quite parallel to the representation given above of the organic structure of the heart. Men do not find watch-springs in nature, but have devised the mode of making them. They have not discovered that the hearts of animals are produced, except as a result of a succession of changes, beginning with the union of the reproductive cells derived from the first parents of every species. Whence, as there is included in the movement of the main-spring of a watch an element derived from human intelligence, so there is, in the contraction of an animal's heart, a something which cannot be identified with the effect of physical force.

Thus the argument from the necessity of something else than organic structure to account for contractility, used in favour of nervous influence, falls to the ground. Nor indeed was there ever any other strong argument. But, though this part of the subject in dispute between WHYTT and HALLER has become obsolete, no professed physiologist can wisely omit to study the controversy carried on between two men so distinguished by extent of knowledge and depth of penetration.

It should be added, that both UNZER and PROCHASKA took the same side with WHYTT, in opposition to HALLER, as to the dependence of the muscular structure

for its contractility on nerves. In his argument against HALLER on this point, UNZER quotes WHYTT'S experiments, and PROCHASKA refers to WHYTT as his leading authority after the following statement:—"It is now placed beyond doubt by many distinguished men, that the irritability of muscles is dependent on the nerves, and cannot exist without them."

The work which has occupied so large a share of attention was published, as before mentioned, in 1751; the two former of the relative treatises, under the name of "Physiological Essays," in 1755, the latter, in the "Edinburgh Essays, Physical and Literary," in 1756. A French translation of the Essays was published in 1759 by THEBAULT.

In the numbers of the "Monthly Review" for March and June of 1752, there is a full analytical review of the Essay, from the close of which the following quotation is taken:—"Thus have we attempted a regular abstract of this curious treatise, which must suffer, in point of force and perspicuity, from any considerable abridgment, and which cannot fail, in the whole, of entertaining our medical and physiological readers. Besides the advantage of an appropriate and extensive erudition, which manifests itself without affectation or pedantry, our author discovers a great natural fund of thinking. His conceptions are happy; his reasoning clear and strong; his expression elegant—free, to the best of our recollection, from the least peculiarity of the Scottish idiom; and so properly adapted to his subject, that it seems to flow of course from his intimate consideration of it."*

In 1752, WHYTT was elected a fellow of the Royal Society of London, to the Transactions of which he afterwards contributed several papers. While he was engaged in these labours, the results of which have been the subject of commentary, considerable changes had taken place in the university. In 1754, MONRO *Secundus*, to whose lectures some who are yet alive have listened, was associated with his father in the Professorship of Anatomy. In the same year WHYTT'S brother-in-law, JAMES BALFOUR of Pilrig, was appointed Professor of Moral Philosophy. In 1755, Dr WILLIAM CULLEN, then Professor of Chemistry in the University of Glasgow, was appointed Professor of Chemistry in Edinburgh, on the failure of Dr PLUMMER'S health. Dr PLUMMER'S course appears to have been in a great measure confined to Chemical Pharmacy. In the "Medical Essays," he published papers—on the Preparation of mercury and antimony, which bears his name; on the analysis of the Moffat water; a case of Hydrophobia. FOTHERGILL says of him—"PLUMMER is no more! He knew chemistry well. Laborious, attentive, and exact; had not a native diffidence veiled his talents as a prelector, he would have been among the foremost in the pupil's esteem: such was the gentleness of his nature; such his universal knowledge, that in any dis-

* Vol. vi. p. 467.

puted point of science the great MACLAURIN always appealed to him as to a living library; and yet so great his modesty, that he spoke to young audiences, upon a subject he was perfectly master of, not without hesitation." PLUMMER died the year after CULLEN was appointed his successor. In 1756, as Dr RUTHERFORD'S health began to decline, he gave up the chemical lectures, which were then divided between MONRO *Primus*, WHYTT, and the new Professor of Chemistry. In the same year Dr THOMAS YOUNG was appointed Professor of Midwifery in the room of Mr ROBERT SMITH, who in 1739 had succeeded Mr JOSEPH GIBSON. Dr YOUNG was unquestionably the founder of the celebrity of the Edinburgh School of Midwifery.

The lectures, with the exception of the lectures on anatomy, were still delivered in Latin, in which language some of the professors are said to have displayed great excellence. SOMERVILLE, in "My Life and Times," bears testimony to the great eloquence in that language of SINCLAIR, and also of PRINGLE, when Professor of Moral Philosophy, whose Latin lecture once a week was, he says, much run after. WHYTT is also reported to have shone in that language. In 1759, ADAM FERGUSON became Professor of Natural Philosophy, nor was he appointed Professor of Moral Philosophy till ten years later. In 1760, BLAIR became Professor of Rhetoric; in 1761, JOHN HOPE was appointed Professor of Botany in the room of ALSTON, who died at an advanced age in the previous year. In 1762, ROBERTSON, the Historian, was appointed Principal in the room of GOLDIE, who had succeeded the younger WISHART in 1736. In 1764, WHYTT'S brother-in-law, JAMES BALFOUR of Pilrig was appointed Professor of the Law of Nature and Nations; he appears to have held this Professorship along with that of Moral Philosophy for a few years, or until ADAM FERGUSON was made Professor of Moral Philosophy in 1769; the other chair he held till 1779, but lived till 1795.

WHYTT'S greatest practical work is his book, "On Nervous, Hypochondriac, or Hysteric Diseases, to which are prefixed some remarks on the Sympathy of the Nerves." This work was published first in the year 1764. While it is a practical work of much value, it is at the same time an extension of the views exhibited in the "Essay on Animal Motions," and an able commentary thereon. In the preface to his work he says, "The design of it is to endeavour to wipe off the reproach, that the name nervous has been bestowed by physicians on all those diseases of the nature and causes of which they are ignorant; and at the same time to show how far the principles laid down in his essay on the vital and other involuntary motions of animals may be of use in explaining the nature of several diseases, and consequently in leading to the most proper method of cure."

The doctrine of sympathy which he lays down in the preliminary dissertation, "On the Sympathy of Nerves," is a corollary to the conclusions of his work on

vital and other involuntary motions. In respect, indeed, to one great order of sympathies, it is a simple extension of that work.

He rejects the opinion that sympathies depend on the continuity of the cellular tissue throughout the body, on the peculiar distribution of the blood-vessels, on the affinities of particular membranes, or on identity of organic structure in remote parts. He refers to WILLIS, as the author of the doctrine that all sympathies are effected through nerves, while he rejects his particular view of all sympathies being dependent on the varied communications of different nerves throughout the body. He maintains that all sympathies take place through the nervous centre by means of nerves; for example, that the tickling of the fauces excites vomiting, not because there is a communication between the nerves of the fauces and the nerves of the stomach, but because the nerves of the part affected by the tickling convey the impression to the nervous centre, whence, by the constitution of the living frame, a motor influence is transmitted through nerves to the many muscular fibres, by the simultaneous action of which vomiting is effected; again, for a second example, an ulcer of the bladder is attended with a pain in the sole of the foot, not because the nerves of these two parts communicate in their course, but because the impression made on the nerves by the irritation of the bladder being carried onwards to the nervous centre, excites an influence by which the nervous fibrils spreading to the sole of the foot are stimulated, so that these two sets of nervous fibrils become similarly affected.

"If it should be objected," he says, "that it is as difficult to account for a sympathy between the nerves, at their origin in the brain, as in their course to the several parts, where they happen to be connected; I answer, that the purpose of these observations is not to explain how the different parts of the body can be endowed, by means of the nerves, either with a sentient or a sympathetic power, but to endeavour to trace the sympathy of the nerves to its true source, which I take to be the brain and spinal marrow."

Sympathies are commonly referred to the two heads of sympathetic acts and sympathetic sensations, while, under the former head, it should be understood, are included not only such acts as vomiting from tickling the fauces, but also acts belonging to the circulation of the blood, secretion, and the vegetative functions in general. Among the examples given by WHYTT falling under what may be called the first order of sympathetic acts, is sneezing from a sudden light falling upon the eye, yawning, and even vomiting, from the sight of a person yawning or vomiting, closing of the eyelids from a loud sound, such as the report of cannon, convulsive movements of the whole body from tickling the soles of the feet. As examples of sympathetic motions of the second order, many are the results of affections of the mind, such as redness and glow of the face from sense of shame, paleness of the face from fear, flow of the tears from grief, abundance of limpid

urine from anxiety of mind; to these may be added, discharge of tears from acrid substances, as mustard in the mouth and throat, the same from acrids affecting the nose, the copious secretion of saliva from the smell or sight of grateful food. Of sympathetic sensations, the examples are, uneasy sensation affecting the teeth from grating sounds, as the noise of a file, pain in the forehead from a draught of cold water, pain of the head from irritation of the stomach, dimness of the eyes from disordered stomach, pain at the top of the shoulder from inflammation of the liver, headache from tight shoes.

It will appear from the case referred to above, in which tickling the fauces is regarded as the cause of vomiting, that WHYTT'S account of sympathetic acts exactly coincides with the view he takes of involuntary motions in his work on that subject. Nor is it to be doubted that that view is substantially correct,—namely, that the peripheral extremity of an afferent nerve being affected by an impression, there results a corresponding condition of the nervous centre, whence, in accordance with the constitution of the living frame, a motor influence is determined, through efferent nervous filaments, to particular organs which are thrown into movement. In short, that sympathetic acts are reflex acts, determined by the nervous centre, on the occurrence of an external impression,—external, that is, to the nervous system. All that is here stated is the general principle; and perhaps it is even now premature to attempt to refer all known sympathies of the nature of vegetative acts to their proper seat, respectively, in the nervous centre. At all events, there is no ground to claim for WHYTT anything more than the general principle now stated, since he makes no attempt to determine the separate endowments of different parts of the encephalon and spinal marrow. The improvement that may be expected on this point will be in proportion as the knowledge increases of the several separate offices assigned to different parts of the nervous system, whether as relates to the central organs or the nervous fibrils. In the mean time, the doubts which exist as to the reality of such orders of nerves as the excito-motory of MARSHALL HALL, and the vaso-motor of BROWN-SEQUARD, tend very much to retard progress. The sympathetic nerve and its ganglia are still a problem; nor should it be too securely assumed that some sympathies belonging to the order of sympathetic acts may not be the result of reflex acts of its ganglia. But this time only can determine. There are innumerable morbid acts, the result of irritations of sensible surfaces, which have never yet been sufficiently inquired into. The tendency, however, at present, among physiologists, seems to be, to deny all source of actual movement to the sympathetic, and to regard its influence as being rather over chemico-vital than over motive acts. Still, whatever may be the final determination, the rule of sympathetic acts, within WHYTT'S expression, will be, that they are determined by impressions acting through nervous centres.

One of the most remarkable morbid sympathies is the occurrence of cramps of

the voluntary muscles, owing to acrimony affecting the mucous membrane of the stomach or bowels, from the simple transitory cramp of the calf of the leg, the result of acid in the stomach, to the violent cramps over the body in the irritation of Asiatic cholera. The dilatation of the pupil from irritation of the bowels may seem to take place through a ganglion of the sympathetic system, since the radiated muscular fibres of the iris are believed to be supplied with nerves from the sympathetic.

But with respect to sympathetic sensations, like the pain of the sole of the foot from an ulcer in the bladder, pain in the cheek and side of the head from a carious tooth, WHYTT doubtless says correctly, that they do not depend on connections of nerves with each other in their course; nevertheless, there is some reason to think that such sympathies may depend on the proximity of the connections of the nervous fibrils concerned at the nervous centre. Thus MÜLLER has remarked, that there is what he calls a radiation of sensation, by which nervous fibrils adjacent in their connection with the nervous centre to that by which an impression is brought thither are similarly affected, so that the sensation is spread not only to the peripheral extremity of the recipient of the impression, but also to the peripheral extremities of all the adjacent fibrils involved at their origin, however distant their peripheral extremities may be from that of the recipient. Thus, if the afferent fibrils from the upper surface of the liver terminate in the nervous centre near the termination of afferent fibrils from the top of the shoulder, a strong impression conveyed through the former will radiate its effect to the adjacent origins of the latter, so as to cause the patient to refer that impression not only to the liver, but also to the top of the shoulder. This explanation gains probability both from the experiments made as to nervous induction, and also from the rule laid down by Sir WILLIAM HAMILTON, that a sentient nervous fibril does not supply a distinct knowledge of the seat of the impression conveyed by it, unless when that impression is moderate; while in proportion as the impression is strong, the knowledge given of its seat is vague.

WHYTT does not indeed suggest this explanation, but he makes a near approach to it, when he says that "the impressions made by the stars by day on the retina are suffocated and lost in those stronger ones made by the illuminated atmosphere." Supplemented, then, by this idea of the radiation of strong impressions, WHYTT's doctrine of sympathetic sensations seems to include all that is at present known on this subject. The late Professor ALISON published in the "Transactions of the Medico-Chirurgical Society of Edinburgh" an excellent commentary on WHYTT's doctrine of sympathy.

The work itself, the preliminary dissertation to which has just been commented on, while it is in every respect worthy of WHYTT's reputation, is hardly of a nature to bear an analysis in a memoir like the present. His illustrations of the symptoms of nervous diseases are given with a masterly hand. They

both afford new proofs of his doctrine of sympathy, and show with how much knowledge, zeal, and ability he strove to throw light on the alarming symptoms brought under his consideration. The whole work bears undeniable evidence that WHYTT was in advance of the age in which he lived, and brings home the conviction, that he stands among those whose labours most largely contributed to the rapid strides of medicine in the latter half of the eighteenth century. This work was translated into French in 1767, by BEGUE DE PRESLE.

One important work remains to be noticed, which was not published till after the author's death. The title of the work referred to is "Observations on the Dropsy in the Brain." WHYTT, in this work, claims for himself an originality which has not been conceded to him undisputed. He enumerates the authors who wrote before him on Hydrocephalus. He shows that HIPPOCRATES, by the term Hydrocephalus, signified water between the membranes and the brain; that later authors, such as MERCURIALIS, WEPFER, BOERHAAVE, have indeed referred to cases of water being found after death within the ventricles of the brain, but had failed to mention the symptoms by which the dropsy of the ventricles of the brain is characterised. He adds, that PETIT had stated that he never found in his dissections water within the cranium anywhere but in the ventricles of the brain, whence he infers the rarity of any other seat of water within the head. He admits that PETIT mentions some of the symptoms which occur in dropsy of the ventricles, but insists that those he refers to are not the most characteristic signs of the early state of the disease. LE DRAN, he says, has described the symptoms of Hydrocephalus internus in such a manner as would lead one to believe he had never seen the disease, unless when conjoined with water between the membranes and the brain. Of Dr DONALD MONRO'S account of Hydrocephalus in his treatise on Dropsy, he remarks, that while he correctly enumerated all the varieties, he has not stated symptoms by which the internal species could be distinguished from the other species, and from other diseases of the head. FOTHERGILL and WATSON published their accounts of this disease two years after WHYTT'S death, but in the same year in which his treatise first appeared in the collected edition of his works. It was considered by WHYTT as a dropsy, nor was it till fifteen years after his death that its inflammatory character was first hinted at by Dr WITHERING. WHYTT'S merit lies chiefly in the correct exhibition of the diagnostic symptoms of the early stages. Of these his account is admirable. It was founded on the observation of twenty cases, in ten of which the presence of the disease was attested by a *post-mortem* inspection. It is hardly to be doubted, then, that he is entitled to the merit of having first brought this disease within the compass of treatment in its earlier stages.

Besides his two great treatises, and the several productions collected together in the edition of his works published in 1768 by his son and Sir JOHN PRINGLE, there are several other detached papers, of which there is a complete list under

the head WHYTT, in WATT's "Bibliotheca Britannica." From that list the following excerpt is taken :—

"On the Difference between Respiration and the Motion of the Heart, in Sleeping and Waking Persons," Essays, Phys. and Lit., vol. i., 1754. "On the Cure of a Fractured Tendo Achillis," ib. "On the Use of Bark in a Dysentery, and a Hoarseness after Measles," ib., vol. iii. "Observations on the Anomalous and True Gout," ib. "Of an Epidemic Distemper at Edinburgh and Southern Parts of Scotland in 1758," Med. Obs. and Inq., vol. ii. "On the Use of Sublimate in the Cure of Phagedenic Ulcers," ib. "Account of an Earthquake felt at Glasgow and Dumbarton; also of a Shower of Dust falling on a Ship between Shetland and Iceland," Phil. Trans. 1755. "On the Remarkable Effects of Blisters in lessening the Quickness of the Pulse in Cough, attended with Infarction of the Lungs and Fever," ib. 1758.

It appears that WHYTT not only carried on a regular correspondence with Sir JOHN PRINGLE on medical subjects, but that he corresponded with physicians and lovers of natural history in several parts of the world. Among others, he corresponded with Dr ALEXANDER GARDEN of Charleston, South Carolina, the same whose name is perpetuated in the beautiful and highly fragrant genus *Gardenia*, garland flowers. GARDEN writes to WHYTT of a new plant which he and Miss COLDEN of New York had simultaneously discovered. It turned out to be a *Hypericum*, or at least a plant since named *Hypericum virginianum*. It is now however named *Elodea virginica*, and in the "Species Plantarum" of Linnæus by WILDENOW, the synonyme is given *Gardenia Coldenia*. Of this plant WHYTT gave an account in the "Edinburgh Essays, Physical and Literary." He published also, in the same work, an account of the vermifuge virtues of the Indian or Carolina pink, the *Spigelia marilandica*, which he derived from his correspondence with Dr JOHN LINING of Charleston. Dr JOHN HOPE afterwards published, in the same work, a larger account of the same plant which he obtained, after WHYTT's death, from Dr LINING. WHYTT published, in the same work, a description of a marine production, "The froth of the sea," after a specimen sent to him by Dr GARDEN. He has given a figure of this body, which he regarded as composed of the semi-developed spawn of a species of *Buccinum*. An account of yellow fever at Charleston, referred to in the above-mentioned catalogue, he derived also from Dr LINING.

In 1762 WHYTT adopted the Pathology of GAUBIUS as his text-book, instead of the Institutes of BOERHAAVE, which he had previously used.

In 1761 he was made first physician to the King in Scotland, an office which is said to have been created for him. In 1763 he was elected President of the Royal College of Physicians of Edinburgh, which office he held at the time of his death.

In the beginning of the year 1765 WHYTT's health began to decline. At first he was affected with diuresis to a very considerable extent, particularly in the night, while there was remarked an inequality of the pulse and occasional depression of spirits. He was confined almost entirely to his house, driving out only on fine days. As the diuresis lessened, a sense of inward oppression and sinking became of frequent recurrence. Spasms of the bowels and flatulent symptoms often arose, along with other marks of great irritability of the alimentary canal. There was cough from the first, which varied much in severity throughout his disease, while, in whatever state the cough was, there was always an abundant, jelly-like spit. It appears that from an early period he had a difficulty of lying on the right side, which, in the latter part of his illness, passed into orthopnœa. His case did not at first seem so serious to his medical friends as it turned out to be. Some slight appearances of gout had manifested themselves just before he became seriously ill. He himself regarded his complaints as connected with gout. The same view was taken by his immediate medical attendants, Dr RUTHERFORD and Dr JOHN CLERK. PORTERFIELD provoked him by pronouncing his complaints hypochondriac. Notwithstanding his sufferings, he continued to take an interest in ordinary topics, and to speak sensibly on these. The continual sense of sinking induced him to take more animal food and wine than his medical advisers approved of. In the summer of 1765 much purple discoloration of his thighs and legs showed itself, as if he had become affected with scurvy, yet there was no coincident affection of the gums. He was put on vegetable diet, and the spots disappeared. Then a paralytic affection seized one arm and one leg; which, however, subsequently became less. His sufferings were great, but the disease did not gain much ground upon him for some time. He was wheeled about his house in a chair. For a short period before his dissolution, one bad symptom succeeded another in quick succession; the sense of sinking, however, disappeared. The cough was severe, and the pulse failed. When within a day or two of his death, anasarca of the lower extremities began, he said calmly to those about him, "The end is come at last;" and two days after, April 15th 1766, he died.

The chest and abdomen were examined after his death. In the cavity of the left pleura there were five pounds of fluid mixed with a substance of gelatinous consistence and bluish colour. In the right cavity there were two pounds of serum. On the left side there were adhesions. There was some fluid in the pericardium. The lungs were free from disease; the heart seemed atrophied. There was a very little water in the abdomen. The viscera were generally free from disease. There was a red spot, the size of a shilling, on the mucous membrane of the stomach. There were concretions in the pancreas.

WHYTT's funeral was public, in so far that the Principal and Professors of the

University, attired in their gowns and preceded by the mace, attended his remains to the grave, while the whole body of the College of Physicians joined in the procession. He was buried in the south burying-ground of the Greyfriars Churchyard, where a tablet with a Latin inscription to him and his wife may still be seen.

A few months before WHYTT's death, Dr JOHN GREGORY had been elected to the Chair of Practice of Medicine on the retirement of RUTHERFORD, whose health now rendered him unable for the duties of the office. It had been expected by Dr CULLEN's friends that this vacancy would have made room for him in that appointment. There was, however, jealousy of CULLEN somewhere, and that arrangement did not take place. On WHYTT's death it seemed certain that CULLEN would become his successor; nevertheless, CULLEN appears at first to have been unwilling to apply for the vacant chair, but he finally consented to become Professor of the Institutes of Medicine.

It has been supposed that CULLEN entertained a jealousy of WHYTT. It has even been pointed out that CULLEN, in the preface to his "First Lines," in making reference to the distinguished followers of STAHL, singles out PORTERFIELD and SIMPSON of St Andrews, far lesser lights, to the total exclusion of WHYTT. But his omission of WHYTT on this occasion only proves how truly he had weighed the doctrines of his former colleague; he refused to account him a disciple of STAHL, knowing that what WHYTT taught of the soul he himself believed. The following passage from CULLEN's "Physiology," as published by Dr JOHN THOMSON, while it reflects the esteem in which CULLEN held his predecessor in the Chair of the Institutes of Medicine, shows the aspect of WHYTT's doctrines, as exhibited in this Memoir, to be in strict accordance with the light in which CULLEN viewed them:—"My late colleague, Dr WHYTT, has, I think, with great strength of argument, shown that the phenomena even of the body itself cannot be explained but upon the supposition of a soul as a sentient principle. It is to this writer that I proposed to refer you; as he considers the argument with a view to the animal economy and to physic, he is the proper authority to be consulted for the demonstration of the immateriality of the soul in the economy, while in a living state. But I must explain more particularly his opinion, and what has been the common one of physicians also. Though they maintain the presence of a soul,—and I think it necessary to the motions that occur in our material part,—yet they are far from considering that there is anything left to it as a rational soul, but that the mutual influence of the soul and body takes place by what may be called a physical necessity: that there is nothing arbitrary in the power of the soul." "And Dr WHYTT, after having occasion to observe several instances of the influence of the mind upon the body, says: 'Nor can we consider the mind as acting either ignorantly or perversely, when it sometimes excites such motions in the body as in-

crease its own pain, and in the end prove more hurtful than beneficial; for these motions do not proceed, as the followers of STAHL have imagined, from any rational views in the mind, or a consciousness that the welfare of the body demands them, but are an immediate consequence of the disagreeable perception which excites it into action.'"* "He is more explicit still," CULLEN continues in his "Treatise upon Vital Motions," "where he considers the share the mind has in producing motions: 'The mind, in carrying on the vital and other involuntary motions, does not act as a rational but as a sentient principle, which, without reasoning, is as certainly determined, by an ungrateful sensation or stimulus affecting the organs, to exert its power in bringing about these motions, as is a scale which, by mechanical laws, turns with the greatest weight.'"† "This," adds CULLEN, "is establishing in the strongest manner a physical necessity in this communication between the mind and body; and I say, indeed, that this has been the most common opinion among physicians."‡ Again, in his notes of a clinical lecture for the month of December 1765, Dr CULLEN, in alluding to the progress which had been made in the investigation of the laws of the nervous system, in the states of health and disease, and to the share which WHYTT had taken in it, observes: "HELMONT, WILLIS, BAGLIVI, WEPFER, HOFFMANN, HALLER, and GAUBIUS, have all done something, but Dr WHYTT more than all, though he has not exhausted the subject, nor removed every difficulty."§ Dr JOHN THOMSON, to whom the medical profession is indebted for the publication of this passage, expresses his own sentiments of WHYTT in the following words:— "Notwithstanding any errors in theory which he may have committed, the writings of this author display an extensive acquaintance with the phenomena of health and disease, habits of accurate observation, and a talent for abstract reasoning and philosophical analysis, not surpassed by any of those who have been employed in investigating the laws by which the economy of living systems is governed."‖

There is nothing which more signally distinguishes WHYTT's writings than clear conception, lucid statement, and sound sense. He never wanders from the point under consideration. He does not indeed refuse to exhibit what he wishes to enforce in several lights, with the view of discovering in which of these it will best gain the reader's assent, but he is never led away from his purpose by any desire of shining, by any love of the fanciful, or by any hankering after paradox. He proceeds straight to the object he has in his mind. His style is well suited to his subject,—pure, unaffected, energetic, and, as one of his English contemporaries before quoted remarks, particularly free from Scottish peculiarities. He

* Works, p. 520.
† Ibid. p. 152.
‡ THOMSON's "Cullen," vol. i. p. 19.
§ THOMSON's "Life of Cullen," i. p. 258.
‖ Ibid., i. p. 258.

unquestionably bestowed great care on his writings, bringing up his narrative in the later editions of his books with much exactness to the improved condition of the subjects at the moment. Hence, considerable differences, always for the better, exist between the first editions of his several writings and the state in which these appear in the collected edition published after his death by his son and Sir JOHN PRINGLE.

In short, WHYTT, though of an ardent temper, really was a man of well-balanced feelings, earnest after truth, not unsolicitous of fame, while all the sentiments he expresses indicate a benevolent turn of mind, full of love to mankind, and a determination, at any cost to himself, to fulfil the duties of his station.

XIII.—*Experimental Inquiry into the Laws of the Conduction of Heat in Bars, and into the Conducting Power of Wrought Iron.* By JAMES D. FORBES, LL.D., D.C.L., F.R.S., V.P.R.S. Ed., Corresponding Member of the Institute of France, Principal of the United College of St Salvator and St Leonard, St Andrews.

(Read 28th April 1862.)

1. The experiments to which I shall have to refer in this paper were all made above ten years ago. A brief report of the general results, so far as they were then obtained, was made to the British Association in 1852,* at whose expense the instrumental apparatus had been provided. The causes of the delay which has occurred in the publication of the numerical results will be presently explained.

2. I had previously, in 1831 and 1832, made some experiments on the conducting power for heat of the metals, with FOURIER's Thermometer of Contact, though merely with a view to determine the exact order of the metals in this respect. The results were not altogether decisive, particularly with regard to the position of Iron in the series; and the paper which was read to the Royal Society of Edinburgh on the 7th January 1833, and which now lies before me, was never published in full. The conclusions are, however, sufficiently stated (without numerical results) in the "Proceedings" of the Society.† The most important conclusion was this:—"That the arrangement of conductors of heat does not differ more from that of conductors of electricity than either arrangement does alone under the hands of different observers." The identity of the two series is now generally admitted, although, at the time I speak of, it had not, I believe, been suspected.

3. In 1850 I commenced experimenting upon iron bars heated at one end, and having thermometers inserted into holes at different points of their length. Experiments in this form had been made by LAMBERT, BIOT, DESPRETZ, and probably by others. But the peculiarity of the series now to be described consists in the manner of reducing the observations, and in the nature of the results sought. In the determinations of preceding authors, the law of communication of heat by conduction within the bar, is *assumed* to be throughout proportional to the rate of the variation of temperature (shown by the thermometers) at each point,‡ reckoned

* Report for 1852 (Belfast), p. 260.

† Vol. i. p. 5 (1833).

‡ This is usually called the Newtonian Law, though it may be doubted how far Newton understood it to apply to internal conduction. (See Phil. Trans. 1701, and Opuscula, tom. ii. p. 419.)

along the axis of the bar, the temperatures having assumed a permanent condition. In conformity with this assumption, it is farther taken for granted that the excesses of temperature of the bar above that of the air may be correctly represented by a logarithmic curve, whose asymptote is parallel to the length of the bar. In order that this may be allowable, it is also necessary to assume that the *exterior loss by radiation* is at every point proportional to the excess of temperature, which (especially when the convective action of the atmosphere is taken into account) is known to be wide of the truth. To *assume* the logarithmic form of the temperature curve, and to compel it to represent the observations, by subjecting the latter to the Procrustean ordeal of the method of "minimum squares," as was done by Biot, might lead, no doubt, to approximate numerical constants, but could never serve to revise and rectify our knowledge of the laws of conduction.* Besides, the results of the statical form of the experiment cannot by itself lead to any estimate of *absolute* conducting power.

4. The plan which I devised in 1850, if not earlier, is plainly specified in the following extracts from letters addressed by me to Professor Kelland in that year, which I shall here transcribe. I may add, that about the same time I transmitted a similar statement to Mr Airy, with a request that it might be preserved among the scientific correspondence of the Royal Observatory. These communications were referred to in a provisional report to the British Association, dated 19th June 1851.†

5. The following is an exact transcript of the portions of the letter first referred to above.

To Professor Kelland.

Phœsdo, by Laurencekirk, 26th Sept. 1850.

* * * * * * *

"I. I propose to find experimentally the effect of external cooling, *from all causes whatsoever*, on the interior temperature of a uniform bar, heated

* The fallacy of the method of minimum squares, as it is often applied to physical inquiries, is well exemplified in this experiment, and may be seen almost at a glance by inspecting Biot's comparative tables of calculated and observed temperatures, given in his *Traité de Physique*, vol. iv., p. 666. The "probable error" of an observation of temperature at each and every point of the bar is considered *as the same;* whereas, when the excesses of temperature are only a few degrees, every experimenter knows that they may be determined with the nicest accuracy, whilst, when the excesses rise to *hundreds* of degrees, the uncertainty is incomparably greater. It is quite evident, that Biot's observations differ *systematically* from the logarithmic law. In referring in 1856 to these observations [*Encyclopædia Britannica*, Sixth Dissertation, Art. (671)], I observed, that "instead of drawing from them, as he does, an argument for the accuracy of the Newtonian law of cooling, the diminution of temperature along the bar is far more rapid at first, and less afterwards, than that law indicates. In fact, the apparent agreement of the formula is owing to the use, in a case to which it does not correctly apply, of that often misapplied rule of the doctrine of chances—the method of least squares." See also Mr Airy's *Theory of Errors of Observations* (1861).

† Report for 1851 (Ipswich). Sections, p. 7.

at one end, in the following way. Take either the very bar to be used, or a shorter one exactly similar; insert in it a thermometer, in all respects as the thermometers are ultimately to be used for finding the *final* temperatures. Heat the *entire* bar, say to 100° C., and observe the *rate of cooling* $\left(\frac{dv}{dt}\right)$, corresponding to given temperatures of the thermometer.

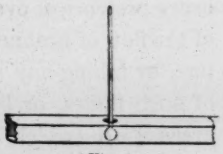


Fig. 1.

There will be evidently no difficulty in doing this with extreme accuracy. I need not insist on the details. We thus obtain for the given bar the proportional expenditure of heat by the surface, depending on the temperature of the axis of the bar, and on nothing else. (By temperature, of course I mean excess of temperature above air.) Let such a table be formed, or a curve constructed.

v	$\frac{dv}{dt}$
100	,,
90	,,
&c.	&c.

I.

6. "II. Let now the bar experiment [for conduction] be made in the usual way, only with greater precautions than perhaps have hitherto been taken (inquiring into the effect of the number and size of the holes, and correcting the thermometers for the unimmersed parts of the scale). Register the final temperatures at different distances from the source of heat, the bar being very long. The curve *ab*, we know, is *nearly* a logarithmic. Project it graphically, and by drawing tangents, ascertain the rate of decrement of temperature for a small increment of length, or $-\frac{dv}{dx}$, for any number of points of the bar,—or else, calculate this, taking thermometric indications by threes [twos], and assuming a modulus for each point. Hence we form a second table, of x, v corresponding, $\frac{-dv}{dx}$, and from Table I., or from a graphical curve of the observations on which it was founded, we add the column of the momentary loss of heat due to all external causes, or $\frac{dv}{dt}$, corresponding to v observed.

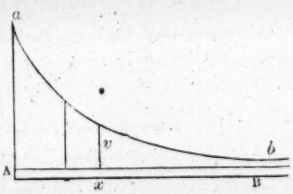


Fig. 2.

x	v	$-\frac{dv}{dx}$	$\frac{dv}{dt}$
1	,,	,,	,,
2	,,	,,	,,
3	,,	,,	,,

II.

7. "III. We may now construct with abscissæ x, and ordinates $\frac{dv}{dt}$, the experimental curve $a\beta$, whose *area will express the total escape of heat from the bar, or from any part of it.* Thus the area $xx'\xi\xi$ will represent the escaped heat from the portion of the bar xx'. I should have no doubt of being able to approximate to this area, with fully the accuracy which the inevitable errors of apparatus and observa-

tions admit of in other respects; and I am particularly anxious to throw aside every pretence of over-refinement. Now, this area is proportional to the difference of the flow of heat across the section of the bar at x and that at x'. We have, therefore, by taking any number of areas, the differences of such fluxes, or by approximating to the whole areas $\xi x \beta$ (which, I think, might quite well be done), we have numbers proportional to the fluxes themselves at x, x', x'', &c. If these numbers are proportional to $\frac{dv}{dx}$ in Table II., the Newtonian law is correct; if otherwise, the deviation will appear, and also the law, or at least numbers giving an empirical law. As I fancy this method to be new and important, pray preserve this letter, and let me know at the same time whether you see any flaw in the argument. You will probably not like the mechanical quadratures; but what else are our experiments but mechanical quadratures in many cases? And this is an experiment on paper capable, I apprehend, of being better measured than an experiment to measure heat with a thermometer.

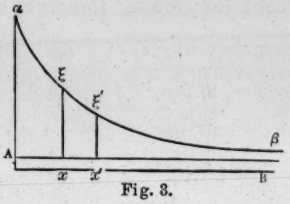


Fig. 3.

8. "IV. It would be a most interesting verification to repeat the whole, with the same bar having a *new* surface given to it, which would radiate twelve or fifteen times faster, which I believe might be done."

9. My correspondent having expressed some doubt as to the accuracy with which the quadratures could be performed, I thus wrote from Phoesdo, on the 11th October 1850: "The mechanical integration I spoke of could be got rid of thus:—Treating a *small part* of the curve ab (in my former letter) as a logarithmic, calculate from it $\frac{d^2v}{dx^2}$ for each point; these numbers should be proportional to the corresponding ordinates of the curve $a\beta$. Nevertheless, I suspect that in practice the method formerly proposed would be more exact." It will be seen presently, that the quadrature of the areas proved to be practicable and accurate, and that it was the method adopted.

10. In a letter, a few weeks later, the conclusions are carried further.

"*To Professor Kelland.*

"*Edinburgh*, 11*th November* 1850.

"In my letter of the 26th September, I explained a form of experiment, by which I proposed to test the fundamental assumption of the Mathematical Theory of Heat. I shall now show that the same experiment performed on bars of different metals, will give at once and directly, the constant of conducting power for each metal, which, so far as I know, has never been done by experiments on bars only, such experiments having given hitherto merely *relative* results. FOURIER

has shown (p. 406) [of the Théorie de la Chaleur] that when the thickness of bars is small, though the temperature of equilibrium varies from the axis to the surface, the temperature at any point is very approximately in a constant ratio to the temperature of the central point of the section to which it belongs; so that the bar may be regarded as a bundle of parallel rods, in each of which the temperature varies with the length according to the same law. Now, the experimental curve $a\beta$ of my last letter represents the momentary loss of heat at every point of the axis of the bar in thermometric degrees; the *area* of the curved space × a unit of transverse section near the axis, represents the number of cubical units of volume of the material, raised 1° by the heat flowing through [across] unit of section, near the axis of the bar at the point x. The specific heat of the metal being known, we can convert this amount of heat or flux across x into absolute measure; for the Flux is $= -K\frac{dv}{dx}$, and $\frac{dv}{dx}$ is known by Table I. of my former letter. Thus every experiment becomes an independent means of finding K, and that without reference to the state of the surface, which has always been a difficulty in comparing different metals with unlike radiating surfaces."

11. The preceding extracts sufficiently set forth the principles on which the experiments were conducted, and I have preferred using the exact words in which I first sketched them, because it serves to fix the date, and also because they were in every point carried out in practice.

12. The experiments referred to in the present paper were all made between the 6th November 1850 and the 17th April 1851. They refer to the conductivity of Wrought-Iron alone. I do not here intend to describe the experiments in detail, nor to enter into the particulars of the reductions (which would require the engraving of some elaborate curves); nor do I offer the final results as thoroughly satisfactory to myself, though I hope, at a future time, to be able to lay before the Society these details, and to carry out the reductions in a way which will leave little to desire in the way of numerical precision.

13. The causes of the delay in publishing the results were these:—The thermometers, on whose exactness and comparability (especially at high temperatures), the accuracy of the experiments materially depended, were furnished to me by a Paris maker, who, *at that time*, enjoyed the highest reputation for trustworthiness and precision. I soon found, however, that some of the instruments were unfit for my purpose, that one or two were disgracefully bad, and that others were not entirely reliable at all points of their scales. The worst thermometers were rejected, and the remainder were used with every possible precaution, and were farther checked by other instruments in my possession. A severe illness in November 1851, before I had resumed my experiments for the winter, brought the series to an abrupt conclusion. Fortunately the data for wrought-iron were

quite complete. The effects of my illness continuing, I contented myself during the succeeding summer (1852) with deducing the best results I then could from the data which I had obtained, trusting to make a more thorough examination of them when the Errors of the Thermometers should be accurately known. The various curves of temperature, velocity of cooling, &c., were most carefully projected on a large scale, and the different stages of reduction carried out precisely as I had projected in my first letter, printed above. The smoothness and regularity of the primary curves (statical and dynamical), of Arts. 5 and 6, left little to desire. The values of the subtangents and various derivative quantities were equalized by the aid of graphical interpolation in the manner so ably used and recommended by Sir JOHN HERSCHEL, in several of his researches. As every step of the reductions was subjected to the test of graphical representation, it is not likely that any material error occurred in the calculation.

14. In October or November 1852 (at Clifton, where I then resided), I brought these provisional calculations to a close, with the results which I shall presently state. I did not abandon the idea of a more rigorous reduction of the observations, and I was fortunate enough to induce the late Mr WELSH, of the Kew Observatory, to undertake the examination of the scales of the principal thermometers used, and especially of that one employed for the highest temperatures, in which a portion of the column being detached, for the purpose of extending the range, the verification was a matter of some delicacy. Mr WELSH's tables of corrections are now in my hands. The application of them will involve some labour, and perhaps the entire reconstruction of the interpolating curves, and a repetition of the calculations depending on them. I confess myself to blame in allowing so many years to elapse without bringing these computations to a satisfactory conclusion, as well as for not extending the observations to other substances, as originally proposed. The state of my health has not been favourable for such tasks, which are necessarily of an irksome and tedious kind. I hope still to execute, at least the desirable revision of the temperatures and reductions. But upon looking again (after some years' interval) at the previous calculations and their results, I feel warranted in publishing both the methods and the conclusions, as worthy of considerable confidence, and as still I believe new. Indeed, I incline to think that I have been perhaps too fastidious in withholding the results obtained, until they should have received the utmost precision which I can give them.

15. As I entertain the idea of publishing a supplement to this paper, with corrected details, I shall not now dwell upon the intermediate steps of the investigation. I shall, however, insert (chiefly from my full journal-notes of 1851) such a description of the apparatus and modes of observation, as may, I hope, materially assist any one in pursuing the experiments for other metals from the point where my labours unfortunately terminated; as it is not very probable that, under present circumstances, they will be resumed by me.

General Account of the Experiments. The Statical Experiment, on the Permanent Temperatures of a long Bar.

16. It has already been stated that these experiments were made in the winter and spring of 1850–51. The material was wrought iron.

17. Some experiments were made on an iron bar one inch square and seven feet long, marked B. But those of which the results will be here given (and which I considered as more trustworthy), were made with a beautiful bar, 1¼ inch square and fully eight feet long, manufactured on purpose for me, and without charge, through the kindness of Mr ROBERT NAPIER of Glasgow. This bar was marked D.

18. It was used in two conditions as to surface,—1*st*, With a bright semi-polished surface, such as that of well-kept steam-engine rods or pistons; 2*d*, After being covered with thin white paper, applied with the least possible quantity of paste. The paper used was what is known in the stationery trade as "tea-paper," and was found to answer the purpose perfectly well. It was employed with the view of testing the conduction of one and the same substance when the radiation of the surface varied in a great proportion (as 1 : 8, according to LESLIE), and also in order to render the surfaces of different bars alike, for which paper is no doubt better fitted than the black varnishes which have been sometimes used for this purpose.*

19. The bar D was heated at one end by means of a cast-iron cup or crucible, finely adjusted to it by filing, and containing melted lead or solder. This was kept in a fluid state, and at as uniform a temperature as possible, by means of a powerful gas furnace. The whole was placed on a table, in a spacious apartment, without a fire, chiefly lighted from the north, and of which the temperature was nearly constant. The bar, with its attached crucible, was supported, at a height of 15 inches above the table, by means of three seasoned mahogany props thinned to an edge above, so as to make the contact with the metal as small as possible. The heated end was maintained at about the temperature of melting lead: by raising the gas flame on the one hand, or by immersing a piece of solid lead in the fluid on the other, the temperature could be regulated with wonderful exactness by my able assistant, Mr JAMES LINDSAY, who acquired great dexterity in the management of the heat, a tedious process, as the experiments lasted six, eight, and even ten hours.† The bar was sufficiently long to prevent the farther end from being sensibly raised in temperature.

* This precaution was a source of considerable trouble to Mr DESPRETZ. See Ann. de Chimie, tom. xix.

† The first thermometer of the series along the bar has to be incessantly watched for this purpose, or, better still, a thermometer with the bulb dipped into the lead in the crucible, kept as near the melting point as possible. So dextrous did my assistant at last become, that for hours this last thermometer was prevented from wavering, even at that high temperature, above a very few degrees of Fahrenheit.

20. Thermometers were inserted in cylindrical holes, drilled in the upper side of the bar. The holes were 0·28 inch diameter. Contact with the thermometer was secured by mercury poured into the colder holes, and an amalgam or fusible metal in a semifluid condition in the hotter ones.* There were usually about ten thermometers inserted in the bar, at distances varying from three inches to eight feet from the zero point at the crucible. Those nearest to the source of heat were defended from its action by two or three interposed polished metal plates, which were found to act efficiently.

21. When after several hours of exposure to steady heat, the bar attained a normal temperature at its various points, the instant was to be seized, when the casual fluctuations became inappreciable, not only on the thermometers nearest to the source, but also in those at a distance. For though the source of heat may, for a time, appear quite steady, the wave of temperature arising from some antecedent irregularity may still be travelling along some remoter portion of the bar. Experience, and the patient entry of a number of successive observations of all the thermometers, can alone secure the desired precision. These experiments must never be made in a hurry.

22. A modification of this mode of observing which occurred to me in the course of these experiments, possesses important advantages, and may be used as a check. I call it the method *by stepping*. One and the same thermometer was transferred to the successive holes in the bar, beginning with the hottest and going on to the coldest, and the temperatures were read in each case. In this mode of operating, each hole was in the first instance provided with its mercury or amalgam, and with its proper thermometer as before; and the thermometer was only withdrawn, and the *stepping thermometer* introduced, when the temperature indicated by the latter (after being held in the hand to cool) reached as nearly as possible the degree known to belong to the hole in which it was to be immersed. It of course took the exact temperature almost instantly, without either heating or cooling the mercury in the hole; and so on to the end. It is an important recommendation of this method, that a single reliable thermometer may be made to perform the whole work, the others being merely used as rough indicators. Nor is there any danger of error arising from slight changes in the temperature of the Source of heat *subsequent* to the commencement of the readings; for experience shows, that the wave of disturbance of temperature advances much slower along the bar than the *stepping* process can be perfectly well gone through. On the whole, this is an immense facility for the extension of such experiments; as

* It was found that when mercury was used for these last, the surface became hotter by convection than the central part of the hole, contrary to the law of the distribution of heat in a solid bar, and consequently an undue (though perhaps hardly sensible) amount of heat was thereby dissipated. I may add, that I ascertained by actual experiment, that the boring of several additional holes between the extreme holes of a bar did not sensibly disturb the conduction of heat when the intermediate holes had thermometers surrounded by mercury inserted in them.

the possession of ten or more reliable thermometers is of itself a condition of success difficultly attainable.

23. In all cases the *free temperature*, or that to be deducted from the readings of the thermometers, in order to get the true excess of statical temperature along the bar, was obtained by inserting a well-compared thermometer into a hole containing mercury drilled in a similar but short bar of iron, supported in the free air of the room in the neighbourhood of the long bar and similarly exposed, but without artificial heat.

Dynamical or Cooling Experiments.

24. These experiments (see Art. 5) are requisite to determine the rate of the superficial dissipation of heat at any point of a bar of given material and section, by the joint influence of radiation and convection. The object is to obtain the "Velocity of Cooling" in terms of the Temperature shown by a thermometer sunk in the bar. For this purpose a short bar of iron marked C was prepared, 1¼ inch square and in other respects similar to the bar D used for the Statical process, except that it was only 20 inches in length. A hole was bored in the centre of one side, into which a thermometer might be introduced with amalgam round it, as in the previous experiments. First of all, a high uniform temperature was communicated to this short bar, in the following way: A cylindrical iron vessel containing fusible metal (four parts of lead, three of tin, and three of bismuth were commonly used) was suspended vertically over a powerful gas-burner, the heat being confined so as to act on the cylinder, by means of an exterior cylindrical chimney, also of iron. The diameter and length of the cylindrical vessel was such as to admit easily the entire bar C, which had a ring at each end, so as to allow it to be more easily introduced and withdrawn by means of a hook. The metal-bath being duly heated and prepared, the experimental bar was first wrapped in several folds of paper, so as to prevent the sudden chill of the fluid metal on its immersion. The bar was then introduced and withdrawn a few times, each end being alternately lowest, so as to equalise the temperature of the bar as much as possible. When hot enough (which is ascertained by a thermometer in the metal bath), it was withdrawn, shaken, and the paper envelope rapidly cut off. The naked bar was then wiped and laid horizontally on two blunt-edged props, so as to stand (as in the case of the other bar) 15 inches above the table. Mercury previously warmed was introduced into the hole or holes (I had usually two or three near the centre of the bar), and thermometers inserted. The temperatures were read off from minute to minute (the time being given by an assistant), and the rate of cooling thus determined. The readings, both in these and all the other experiments, were made by myself, with exceptions too trifling to require notice, and the rate of cooling was deduced graphically or by calculation.

25. The Statical and Dynamical forms of experiment on the relations of a given bar to heat (*both* of which are required for the solution of our problem), each present their peculiar difficulties. But there is one difficulty in connection with the latter, which I think it right to mention, because I did not succeed in wholly removing it when temperatures approaching 200° Cent. (392° Fahr.) are to be employed. The object of the *Dynamical* experiment (as already explained), is to find the rate at which any point of the bar in the *Statical* experiment is, in point of fact, parting with its heat by the surface, ascertained in terms of the temperature shown by the thermometer sunk into it at that point. But when a bar has been *uniformly* heated in all its parts by immersion in the metal-bath, the distribution of heat over any transverse section is not at first the same as when the bar in the statical experiment has attained a permanent temperature; nor the same as when the bar under experiment has cooled to a certain extent. In fact, FOURIER's analysis shows, that in the early stages of cooling of a body at first heated uniformly, the temperature includes in its expression certain circular functions, which, by and by (and in good conductors very rapidly), become insensible. Such oscillations affecting the *rate of cooling* $\left(\text{or } -\frac{dv}{dt}\right)$ are perceptible in the experiments which I have made, and influence the determination of this important quantity in the highest part of the scale. The general tendency of the effect is manifestly to make the rate of cooling of the thermometer, sunk to the axis of the bar, at first *too small*. For the bar being uniformly heated from centre to surface when it is withdrawn from the metal-bath, it is only as the superficial parts cool that the central parts begin to lose heat, which they supply to the surface. Next, the rate of superficial cooling will be *relatively* somewhat accelerated, and a fresh demand upon the interior will occur. These gushes of heat will gradually disappear, and, as I have said, are in good conductors only at first perceptible.*

26. The initial irregularities at temperatures approaching 200° Cent. are the greatest difficulties which met me in this inquiry, and I fear that they have been but partially overcome. We are precluded from obviating them by the natural expedient of heating the bar initially to a much higher temperature, and allowing it to cool spontaneously down to that at which the observations commence. For even polished iron changes the condition of its radiating surface at temperatures which considerably exceed 200° Cent., so that this plan is inadmissible. In fact, the heat was pushed in these experiments quite to the limit.† I have fully stated this difficulty, in order that future experimenters may be prepared to contend with it. Electroplating might possibly succeed, though I fear not.

* This interpretation of the physical origin of the periodic functions in the cooling of bodies was given in the *Encyclopædia Britannica*, Sixth Dissertation, Art. (674). When the circular functions have exhausted themselves, the exponential portion of the expression for the temperature alone remains. See FOURIER, *Théorie Analytique de la Chaleur*.

† When coated with paper, the paper begins to singe at a temperature slightly above that of melting tin, or 442° Fahr.

The Reductions.

27. The fundamental observations are those on the permanent temperatures of a long iron bar heated at one end, and measured by thermometers placed at fixed intervals along the bar (Art. 16, &c.). These were projected in a curve (which approximates to, and has usually been treated as a logarithmic curve), of which the length of the bar is the axis, and the temperatures (or rather excesses of temperature above the surrounding space) the ordinates. This curve of statical temperature was verified in all its parts as follows:—The first projection was made from one of the completest and most satisfactory of the sets of observations, including the temperature of ten or eleven points along the bar. Other sets of observations were recorded, in which the fundamental temperatures differed more or less from these, the source of heat being either more or less intense. But each group of observations, while it may be treated separately, may also be regarded as belonging *to distinct parts of one common curve* (so long as the bar and its surface are unchanged). We are thus enabled to interpolate temperatures not corresponding to observed points in the primary experimental curve, and to verify them indefinitely. This process of interpolating independent groups of observations is peculiarly suited to the graphical method of reduction, and has been pursued with entire success.

28. Tangents were drawn and subtangents measured, and also calculated from ordinates, for different points of the statical curve. The table of $\frac{dv}{dx}$ (where v is the excess of temperature of any point of the bar at a distance x from the origin at the heated end) is thus formed (see Art. 6). The values being again projected in terms of v, are graphically equalised.

29. Next, the dynamical experiment of the cooling of the short bar (Art. 24) is treated by projection; the temperatures being laid out in a curve in terms of the time (t.) From the tangents to this curve the rate of cooling $\left(\frac{dv}{dt}\right)$, in terms of v, is calculated, and also projected and equalised.

30. These last numbers are erected as ordinates upon the original line representing the axis of the long bar in the statical experiment, as in fig. 3, Art. 7. The perpendiculars are the values of $\frac{dv}{dt}$, in terms of the ascertained measure of v at each point along the bar. The *area* of this last curve between the ordinate corresponding to x and infinity, represents the total loss of heat from the surface of the bar on the cooler side of x, and consequently *the whole flux of heat across the section at x, since that flux is the quantity of heat necessary to compensate the whole loss of heat from the surface of the bar beyond x*, the bar being, by hypothesis, in a permanent condition. The determination of this area by the quadrature of the successive parts is not difficult, as this curve also approximates to a logarithmic, whence the area between successive ordinates, and also that adjoining the asymptote, can be calculated with sufficient exactness.

31. By comparing the flux of heat thus found, across any section of the bar, with the value of $\frac{dv}{dx}$ for that section, we can test the accuracy of the law of conduction as usually assumed. We can also ascertain, with reference to the units of measure employed, the exact conductivity of the bar, whether constant or varying with temperature.

32. The processes above mentioned having been gone through with great care, partly in 1851, but principally in the summer of 1852, I arrived at the following conclusions, which I transcribe *literally* from my note-book of the last named year. It is to be observed, that two entirely independent series of observations are here compared—those made with the naked iron bar marked D, or that which had its surface moderately polished (Art. 18), and those with the same bar when covered with "tea paper," in which case the forms of the curves are altogether altered, owing to the increased radiation of the surface.

Memorandum of 1852.*

"Results of preceding investigations.

33. "A. For naked $1\frac{1}{4}$ inch iron bar.

Actual Temperature. Centigrade.	Excess above Air.	$\frac{dv}{dx}$ from Diag. XIII.	Total Flux (F) of Heat, corresponding to $\frac{dv}{dx}$, by Diag. XVII.	$\left(\frac{F}{\frac{dv}{dx}}\right)$, proportional to K.†
25°	12°	11	·15	·0136
50	37	40	·52	·0130
75	62	71	·93	·0131
100	87	104	1·31	·0126
125	112	142	1·71	·0122
150	137	187	2·08	·0112
175	162	244	2·43	·0100
200	187	310	2·72	·00875

34. "B. For covered $1\frac{1}{4}$ inch iron bar.

Temperature.	Excess.	$\frac{dv}{dx}$ Diag. XIV.	Flux. Diag. XVIII.	$\left(\frac{F}{\frac{dv}{dx}}\right)$
25°	12°	15°	·22	·0147
50	37	52	·72	·0138
75	62	92	1·13	·0123
100	87	138	1·56	·0113
125	112	187	2·00	·0107
150	137	235	2·45	·0107
175	162	295	3·00	·0102

* The diagrams referred to in this memorandum are all drawn out, but are not engraved pending the revision of the scales of the thermometers.

† K expresses the absolute conducting power in terms of the thermal capacity of water.

35. "FINAL RESULT.—The projections of both the observed series of numbers representing the conductivity in terms of the temperature projected in Diagram XIX., give a value of $\frac{F}{\frac{dv}{dx}}$ pretty regularly decreasing from ·0150 at 0° C. to ·0092 at 200°."

36. In order to reduce the unit in the last column of these tables to the absolute unit K, which expresses the amount of heat necessary to raise one cubic foot *of water* by 1° Cent., we must multiply the numbers in the final columns by the specific heat of iron, and also by its specific gravity. These numbers are respectively 0·114 (REGNAULT) and 7·79. Their product is 0·888. Assuming the uniform decrease of the conductivities with increasing temperature, and making use of *both* the series,—as indicated at the close of the preceding paragraph,—and adapting them to the water unit, we obtain the following *approximate* numbers. The numbers in the second column refer to the following units—the English foot, the minute, and the centigrade degree.* The third column has the *centimetre* substituted for the foot.†

Temperature, Centigrade.	Conducting Power of Wrought Iron.	
	Units: the Foot, Minute, and Centigrade Degree.	Units: the Centimetre, Minute, and Cent. Degree.*
0°	·0133	12·36
25°	·0127	11·80
50°	·0120	11·15
75°	·0114	10·59
100°	·0107	9·94
125°	·0101	9·38
150°	·0094	8·73
175°	·0088	8·18
200°	·0082	7·62

37. It will be recollected that these numbers are offered as approximate only.

* Or (more fully) it expresses in centigrade degrees the temperature communicated to a cubic foot of water in one minute, across a plate of iron one foot thick, whose surfaces are maintained at a constant difference of temperature of one degree centigrade.

† This is perhaps the most convenient unit of conductivity for general use. The original thermal unit of FOURIER (who first gave a correct definition of this quantity) was referred to the minute and the metre as the units of time and length, to the interval from the freezing to the boiling point of water as the unit of temperature and the unit of heat was the quantity required to melt one kilogramme of ice (*Théorie*, Arts. 68, 69). It is plain from Art. 59, and others of the same work, that FOURIER had no idea that the conductivity varied with the actual temperature—an admission which must be held to leave the Newtonian law true *in form* only, since the flux is proportional in any one substance not to $\frac{dv}{dt}$ only, but is also a direct function of v.

When the revision of the scales of the thermometers has been applied to the results (which will necessitate the re-construction of the diagrams), I intend to publish the final results for iron in a sequel to this paper.

38. The following consideration, however, as it encouraged me to go through the labour of the provisional calculation, makes me believe that the results will not be materially altered. Indeed, it is one of the advantages of the method here employed, to render us in some degree independent of the precision of the thermometers. Since the same thermometers were carefully used in both the statical and dynamical experiments, the outstanding errors of temperature, as read off, will affect both results, if not exactly to the same amount, at all events in the same direction. The final columns of the tables of page 144, containing the conducting powers, depend on the ratio of two quantities—the flux of heat (F) at any point, depending partly on the dynamical, partly on the statical experiment, and the differential co-efficient $\frac{dv}{dx}$, depending wholly on the statical experiment. The thermometric error will make both these quantities too great, or else both too small, and the ratio will be but slightly affected.

St Andrews, 21*st April* 1862.

Postscript.

The diminishing conducting power of iron with increased temperature harmonises with similar facts in electricity, thus adding a fresh analogy to those adverted to in Art. 2 of the preceding paper. According to the recent experiments of Arndtsen (*Poggendorff's Annalen*, May 1858), the resistance to the transmission of electricity through iron is increased nearly one-half by a rise of temperature of 100° Cent.

28*th April* 1862.

XIV.—*On the Density of Steam.* By W. J. Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edin., &c.

(Read 28th April 1862.)

1. The object of the present paper is to draw a comparison between the results of the mechanical theory of heat, and those of the recent experiments of Messrs Fairbairn and Tate on the density of steam, published in the "Philosophical Transactions" for 1860.

General Equation of Thermodynamics.

2. The equation which expresses the general law of the relations between heat and mechanical energy in elastic substances was arrived at independently and contemporaneously by Professor Clausius and myself, having been published by him in Poggendorff's "Annalen" for February 1850, and communicated by me to the Royal Society of Edinburgh, in a paper which was received in December 1849, and read on the 4th of February 1850. The processes followed in the two investigations were very different in detail, though identical in principle and in results; Professor Clausius having deduced the law in question from the equivalence of heat and mechanical energy as proved experimentally by Mayer and Joule, combined with a principle which had been previously applied to the theory of substantial caloric by Sady Carnot, while by me the same law was deduced from the "hypothesis of molecular vortices," otherwise called the "centrifugal theory of elasticity."

3. Although, since the appearance of the paper to which I have referred, the notation of the general equation of thermodynamics has been improved and simplified in my own researches, as well as in those of others, I shall here present it, in the first place, precisely in the form in which I first communicated it to this Society, in order to show the connection between that equation in its original form, and the law of the density of steam, which has since been verified by the experiments of Messrs Fairbairn and Tate. The equation, then, as it originally appeared in the twentieth volume of the "Transactions of the Royal Society of Edinburgh," p. 161, is as follows:—

$$\delta Q' = -\frac{\tau - \varkappa}{Cn\mathrm{M}}\left\{\delta \mathrm{V}\left(\frac{1}{\mathrm{V}} - \frac{d\mathrm{U}}{d\mathrm{V}}\right) - \delta\tau\frac{d\mathrm{U}}{d\tau}\right\}; \quad . \quad . \quad . \quad . \quad (1.)$$

in which the symbols have the following meanings:—

τ, the absolute temperature of an elastic substance as measured from the *zero of gaseous tension*, a point which was then estimated to be at

274°·6 Cent. below that of melting ice, but which is now considered to be more nearly at 274° Cent., or 493°·2 Fahr. below that temperature.

κ, A constant, expressing the height on the thermometric scale of the temperature of total privation of heat above the zero of gaseous tension. This constant was then only known to be very small; according to later experiments, it is either null or insensible.

nM, The ideal or theoretical weight, in the perfectly gaseous state, of an unit of volume of the substance, under unity of pressure, at the temperature of melting ice.

C, The absolute temperature of melting ice, measured from zero of gaseous tension (that is to say, according to the best existing data, C = 274° Cent., or 493°·2 Fahr.)

V, The actual volume of unity of weight of the substance.

δV, An indefinitely small increment of that volume.

$\delta\tau$, An indefinitely small increment of temperature.

U, A certain function of the molecular forces acting in the substance.

$+\delta Q'$, The quantity of heat which *appears*, or $-\delta Q'$, the quantity of heat which *disappears* during the changes denoted by δV and $\delta\tau$, through the actions of molecular forces, independently of heat employed in producing changes of temperature; such quantity of heat being expressed in equivalent *units of mechanical energy*.

The equation having been given in the above form, it is next shown (in the same volume, p. 163), that the differential co-efficients of the function U have the following values:—

$$\frac{dU}{dV} = \frac{1}{V} - CnM\frac{dP}{d\tau}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

$$\frac{dU}{d\tau} = -\frac{\varkappa}{\tau^2} - CnM\int dV \cdot \frac{d^2P}{d\tau^2}. \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

4. The physical law of which the general equation just cited is the symbolical expression, may be thus stated in words:—*The mutual transformation of heat and mechanical energy during any indefinitely small change in the density and temperature of an elastic substance, is equal to the temperature, reckoned from the zero of absolute cold, multiplied by the complete differential of a certain function of the pressure, density, and temperature; which function is either nearly or exactly equal to the rate of variation with temperature of the work performed by indefinite expansion at a constant temperature.*

5. It may be remarked, that the quantity,—

$$\varphi = \text{k hyp. log. } \tau + \frac{1}{CnM}(\text{hyp. log. V} - \text{U}) = \text{k hyp. log. } \tau + \int \frac{dP}{d\tau} dV \quad . \quad . \quad (4.)$$

(k being the real specific heat of the substance in units of mechanical energy), is what, in later investigations, I have called the "*thermody-*

namic function;" and that by its use, and by making $\kappa=0$, equation 1 is reduced to the simplified form,

$$-\delta Q = \tau\delta\varphi - \mathrm{k}\,\delta\tau; \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

but the following notation is more convenient: Let δh denote the *whole heat absorbed by the substance*, not in units of mechanical energy, but in ordinary thermal units, and J the value of an ordinary thermal unit in units of mechanical energy, commonly called "JOULE's Equivalent," so that

$$\mathrm{J}\delta h = \mathrm{k}\delta\tau - \delta Q;$$

then the general equation of thermodynamics takes the form

$$\mathrm{J}\delta h = \tau\delta\varphi. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

6. For the purposes of the present paper, the most convenient form of the thermodynamic function is that given in the second line of Equation 4; but it may nevertheless be stated, that in a paper read to this Society in 1855, and which now lies unpublished in their archives, it was shown that another form of that function, viz.

$$\varphi = \left(\mathrm{k} + \frac{\mathrm{P}_0\mathrm{V}_0}{\mathrm{C}}\right)\text{hyp. log }\tau - \int\frac{d\mathrm{V}}{d\tau}d\mathrm{P}, \quad . \quad . \quad . \quad . \quad (7.)$$

was useful in solving certain questions; $\frac{\mathrm{P}_0\mathrm{V}_0}{\mathrm{C}}$ denoting the same thing with $\frac{1}{Cn\mathrm{M}}$ in Equation 1.

Application of the General Equation of Thermodynamics to the Latent Heat and Density of Steam.

7. At the time when the general equation (1.) was first published, sufficient experimental data did not exist to warrant its application to the computation of the density of a vapour from its latent heat. But very soon afterwards, various points, which had previously been doubtful, were settled by the experiments of Mr JOULE and Professor WILLIAM THOMSON; and in particular Mr JOULE, by his experiments published in the "Philosophical Transactions" for 1850, finally determined the exact value of the mechanical equivalent of a British unit of Heat, to which he had been gradually approximating since 1843—viz.,

$$\mathrm{J} = 772 \text{ foot-pounds};$$

and Messrs JOULE and THOMSON in 1851, 1852, and 1853, made experiments on the free expansion of gases, especially dry air, and carbonic acid, which established the very near, if not exact, coincidence of the true scale of absolute temperature with that of the perfect gas-thermometer; that is to say, those experiments proved that κ in the equation (1.) is sensibly $=0$. When, with a knowledge of these facts, equation (1.) is applied to the phenomenon of the evaporation of a liquid

under a constant pressure, and at a constant temperature, it takes the following form:—

$$Jh = \tau \frac{dP}{d\tau} (V - v) \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

where

J denotes JOULE's equivalent, or 772 foot-pounds per British unit of heat (a degree of Fahrenheit in a pound of liquid water);

h, The heat which disappears during the evaporation of 1 lb. of the liquid; that is, its *latent* heat of evaporation in British units:—

τ, The absolute temperature (=temperature on Fahrenheit's scale + 461°2 Fahr.).

P, The pressure under which the evaporation takes place.

V, The volume of 1 lb. of the vapour.

v, The volume of 1 lb. of the liquid.

As the latent heat of evaporation of various fluids is much more accurately known by direct experiment than the volume or density of their vapours, the most useful form in which the equation (8.) can be put, is that of a formula for computing the volume of a vapour from its latent heat, viz.:—

$$V = v + \frac{Jh}{\tau \frac{dP}{d\tau}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

8. Results of this formula were calculated by Messrs JOULE and THOMSON, and by Professor CLAUSIUS for steam, and showed, as had been expected, a greater density and less volume than the law of the perfectly gaseous condition would give. Some results of the same kind, and leading to the same conclusion, were also computed by me and published in the "Philosophical Transactions" for 1853–54 But for some years no attempt was made by any one to make a table for practical use of the volumes of steam from equation (9.); because the scientific world were in daily expectation of the publication of direct experimental researches on that subject by M. REGNAULT.

9. At length, in the spring of 1855, having occasion to deliver, to the class of my predecessor, Professor GORDON, a course of lectures on the mechanical action of heat, and finding it necessary to provide the students with a practical table of densities of steam founded on a more trustworthy basis than the assumption of the laws of the perfectly gaseous condition, I ventured upon the step of preparing a table of the densities of steam for every eighteenth degree of Fahrenheit's scale, from 86° to 410° inclusive, with the logarithms of those densities and their differences, arranged so as to enable the densities for intermediate temperatures to be calculated by interpolation. Those tables were published in a lithographed abstractof the course of lectures before mentioned, which is now out of print. The same tables, however, have since been revised, and extended to every

ninth degree of Fahrenheit, from 32° to 428°, and have been printed at the end of a work, "On Prime Movers." An account of the original tables was read to the British Association in 1855.*

10. In the unpublished paper before mentioned, as having been read to this Society in 1854, the densities of the vapours of ether and bisulphuret of carbon, under the pressure of one atmosphere, as computed by equation (9.), are shown to agree exactly with those calculated from the chemical composition of those vapours.

11. The method of using equation (9.), to calculate the volume of one pound of steam, is as follows:—

I. Calculate the *total heat of evaporation of steam* from 32°, at a given temperature T° on Fahrenheit's scale, by REGNAULT'S well-known formula $1091{\cdot}7 + 0{\cdot}305\ (T° - 32°)$. . . . . (10.)

II. From that total heat subtract the heat required to raise one lb. of water from 32° to T° Fahr., viz.,

$$\int_{32°}^{T} c dT;$$

c being the specific heat of water; the remainder will be the latent heat of evaporation of one lb. of steam at T°; that is to say,

$$h = 1091{\cdot}7 + 0{\cdot}305\ (T - 32°) - \int_{32°}^{T} c dT. \quad . \quad . \quad . \quad . \quad (11.)$$

In computing the value of the integral in this formula, use has been made of an empirical formula founded on M. REGNAULT'S experiments on the specific heat of water, as to which, see the "Transactions" of this Society for 1851, viz.:—

$$\int_{T'}^{T} c dT = T - T' + 0{\cdot}000000103\{(T - 39°{\cdot}1)^3 - (T' - 39°{\cdot}1)^3\}. \quad . \quad . \quad (11\text{-A.})$$

III. The absolute temperature is given by the formula,

$$\tau = T + 461°{\cdot}2 \text{ Fahr.}, \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

IV. The value of $\tau \frac{dP}{d\tau}$ is deduced from the following formula, first published in the "Edinburgh Philosophical Journal" for July 1849:—

$$\text{com. log. } P = A - \frac{B}{\tau} - \frac{C}{\tau^2}; \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

from which it follows that

$$\tau \frac{dP}{d\tau} = 2{\cdot}3026\ P \left(\frac{B}{\tau} + \frac{2C}{\tau^2}\right); \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

* The reason for using 9° Fahr. as the interval of temperature is, that it is equal to 5° Centigrade and to 4° Reaumur; so that the tables can be applied with ease to any one of those three scales.

the values of the constants for steam being,—

A, for pounds of pressure on the square foot, . . 8·2591;
log. B for Fahrenheit's scale . . . . =3·43642;
log. C „ „ . . . . =5·598$\bar{7}$3.

V. The volume of a cubic foot of liquid water at the temperature T may be computed with sufficient accuracy for the present purpose by the following formula:—

$$v \text{ nearly} = 0{\cdot}00801\left(\frac{\tau}{500}+\frac{500}{\tau}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

VI. These preliminary calculations having been made, the formula 9 can now be applied to the calculation of the volume of one pound of steam (making J=772); and by this process the tables already mentioned were computed.

Comparison of the Results of Theory, with those of Messrs FAIRBAIRN *and* TATE's *Experiments.*

12. The experiments of Messrs FAIRBAIRN and TATE on the density of steam are described in a paper which was read to the Royal Society of London, as the Bakerian lecture, on the 10th of May 1860, and published in the "Philosophical Tranactions" for that year. The results of those experiments give what is called the "*relative volume*" of steam: that is, the ratio which its volume bears to that of an equal weight of water at the temperature of greatest density, 39°·1 Fahr.; but in the following table of comparison, each of those relative volumes is divided by 62·425, the weight of a cubic foot of water at 39°·1 in lbs., so as to give the volume of one lb. of steam in cubic feet. The numbers of the experiments are the same as in the original paper; those made at temperatures below 212° being numbered from 1 to 9, and those made at temperatures above 212° from 1′ to 14′.

Comparison of the Theory, with the Experiments of Messrs Fairbairn *and* Tate.

Number of Experiment.	Temperature. Fahrenheit.	Volume of one lb. of Steam in Cubic Feet.		Difference.	Difference ÷ Exper. Vol.
		By Theory.	By Exper.		
1.	136·77	132·20	132·60	−0·40	$-\frac{1}{330}$
2.	155·33	85·10	85·44	−0·34	$-\frac{1}{250}$
3.	159·36	77·64	78·86	−1·22	$-\frac{1}{65}$
4.	170·92	60·16	59·62	+0·54	$+\frac{1}{110}$
5.	171·48	59·43	59·51	−0·08	$-\frac{1}{744}$
6.	174·92	55·18	55·07	+0·11	$+\frac{1}{501}$
7.	182·30	47·28	48·87	−1·59	$-\frac{1}{30}$
8.	188·30	41·81	42·03	−0·22	$-\frac{1}{191}$
9.	198·78	33·94	34·43	−0·49	$-\frac{1}{70}$
1′.	242·90	15·61	15·11	+0·50	$+\frac{1}{30}$
2′.	244·82	14·77	14·55	+0·22	$+\frac{1}{66}$
3′.	245·22	14·67	14·30	+0·37	$+\frac{1}{39}$
4′.	255·50	12·39	12·17	+0·22	$+\frac{1}{55}$
5′.	263·14	10·96	10·40	+0·56	$+\frac{1}{19}$
6′.	267·21	10·29	10·18	+0·11	$+\frac{1}{93}$
7′.	269·20	9·977	9·703	+0·274	$+\frac{1}{35}$
8′.	274·76	9·158	9·361	−0·203	$-\frac{1}{47}$
9′.	273·30	9·367	8·702	+0·665	$+\frac{1}{13}$
10′.	279·42	8·539	8·249	+0·290	$+\frac{1}{28}$
11′.	282·58	8·145	7·964	+0·181	$+\frac{1}{44}$
12′.	287·25	7·603	7·340	+0·263	$+\frac{1}{28}$
13′.	292·53	7·041	6·938	+0·103	$+\frac{1}{66}$
14′.	288·25	7·494	7·201	+0·293	$+\frac{1}{24}$

Remarks on the Differences between the Theoretical and Experimental Results.

13. The differences between the theory and the experiments as to the volumes of steam at temperatures below 212°, are, with few exceptions, of very small relative amount; and they are at the same time so irregular, as to show that they must have mainly arisen from causes foreign to the data used in the theoretical computations.

14. Above 212° also, the differences show irregularity, especially in the case of experiments 8′ and 9′, where a *fall* of temperature is accompanied by a diminution instead of an increase in the volume of one pound of saturated steam, as determined by experiment. But still those differences, presenting as they do, in every case but one, an excess of the theoretical above the experimental volume, show that some permanent cause of discrepancy must have been at work; although they may not be regular enough to determine its nature and amount, nor large enough to constitute errors of importance in practical calculations relating to steam-engines.

15. So far as it is possible to represent those differences by anything like a formula, they agree in a rough way with a *constant excess* of about 0·27 of a cubic foot in the theoretical volume of one pound of steam above the experimental volume; and this also represents in a rough way the difference between the curves, whose ordinates express respectively the results of the theoretical formula and those of an empirical formula deduced from the experiments, so far as those curves, as shown in a plate annexed to the paper referred to, extend through the limits of actual experiment on steam, above 212°.

16. As the principles of the mechanical theory of heat may now be considered to be established beyond question, it is only in the data of the formula that we can look for causes of error in the theoretical calculation. I shall now consider those data, with reference to the probability of their containing numerical errors.

I. *Total Heat of Evaporation.*—It is very improbable that this quantity, as computed by M. REGNAULT's formula, involves any material error.

II. *Sensible Heat of the Liquid Water.*—The empirical formula from which this quantity is computed was determined from experiments by M. REGNAULT, which agree extremely well amongst themselves. (For the investigation of the formula, see "Trans. Royal Soc. Edin.," vol. xx. p. 441). The subtraction of the sensible heat from the total heat leaves the latent heat, upon which the increase of volume depends; hence, to account for an error in excess of the formula for the volume by means of an error in the computation of the sensible heat, it must be supposed that the specific heat of liquid water above 212° increases much more rapidly than M. REGNAULT's experiments show, so as to produce a correspondingly more rapid diminution in the latent heat of evaporation. It is easily computed, for example, that to account for an error in excess of 0·27 of a cubic foot in the volume of one pound of steam at 266°, by an error in defect in the sensible heat, we must suppose that error to amount to about 24 British thermal units per pound of water; but such an error is altogether improbable.

III. *Absolute Temperature.*—The position of the absolute zero may be considered as established with a degree of accuracy which leaves no room for any error sufficient to account for the differences now in question.

IV. *Function* $\tau \frac{dP}{d\tau}$.—The same may unquestionably be said of this function; which represents the mechanical equivalent of the latent heat of evaporation of so much water as fills one cubic foot more in the vaporous than in the liquid state.

V. *The Volume of one Pound of the Liquid Water* is itself too small to affect the question.

VI. The received value of the *Mechanical Equivalent of a unit of Heat* cannot err by so much as $\frac{1}{300}$th part of its amount.

Conclusions.

17. It appears, then, that none of the data from which the theoretical calculations are made are liable to errors of a magnitude sufficient to account for the differences between the results of those calculations and the results of Messrs FAIRBAIRN and TATE's experiments, small as those differences are in a practical point of view. Neither does there appear to have been any cause of error in the mode of making the experiments. There remains only to account for those differences, the supposition that there was some small difference of molecular condition in the steam whose density was measured in the experiments of Messrs FAIRBAIRN and TATE, above 212°, and the steam whose total heat of evaporation, as measured by M. REGNAULT, is the most important of the data of the theoretical formula,—a difference of such a nature as to make a given weight of steam in Messrs FAIRBAIRN and TATE's experiments occupy somewhat less space, and therefore require somewhat less heat for its production, than the same weight of steam in M. REGNAULT's experiments at the same temperature. That difference in molecular condition, of what nature soever it may have been, was in all probability connected with the fact, that in the experiments of Messrs FAIRBAIRN and TATE, the steam was at rest, whereas in those of M. REGNAULT it was in rapid motion from the boiler towards the condenser. It is obvious, however, that in order to arrive at a definite conclusion on this subject, further experimental researches are required.

XV.—*On the Secular Cooling of the Earth.* By Professor William Thomson, LL.D., F.R.S., F.R.S.E. (Plate VIII.)

(Read 28th April 1862.)

1. For eighteen years it has pressed on my mind, that essential principles of Thermo-dynamics have been overlooked by those geologists who uncompromisingly oppose all paroxysmal hypotheses, and maintain not only that we have examples now before us, on the earth, of all the different actions by which its crust has been modified in geological history, but that these actions have never, or have not on the whole, been more violent in past time than they are at present.

2. It is quite certain the solar system cannot have gone on, even as at present, for a few hundred thousand or a few million years, without the irrevocable loss (by dissipation, not by *annihilation*) of a very considerable proportion of the entire energy initially in store for sun heat, and for Plutonic action. It is quite certain that the whole store of energy in the solar system has been greater in all past time than at present; but it is conceivable that the rate at which it has been drawn upon and dissipated, whether by solar radiation, or by volcanic action in the earth or other dark bodies of the system, may have been nearly equable, or may even have been less rapid, in certain periods of the past. But it is far more probable that the secular rate of dissipation has been in some direct proportion to the total amount of energy in store, at any time after the commencement of the present order of things, and has been therefore very slowly diminishing from age to age.

3. I have endeavoured to prove this for the sun's heat, in an article recently published in "Macmillan's Magazine,"* where I have shown that most probably the sun was sensibly hotter a million years ago than he is now. Hence, geological speculations assuming somewhat greater extremes of heat, more violent storms and floods, more luxuriant vegetation, and hardier and coarser grained plants and animals, in remote antiquity, are more probable than those of the extreme quietist, or "uniformitarian" school. A "middle path," not generally safest in scientific speculation, seems to be so in this case. It is probable that hypotheses of grand catastrophes destroying all life from the earth, and ruining its whole surface at once, are greatly in error; it is impossible that hypotheses assuming an equability of sun and storms for 1,000,000 years, can be wholly true.

* March 1862.

4. FOURIER's mathematical theory of the conduction of heat is a beautiful working out of a particular case belonging to the general doctrine of the "Dissipation of Energy."* A characteristic of the practical solutions it presents is, that in each case a distribution of temperature, becoming gradually equalised through an unlimited future, is expressed as a function of the time, which is infinitely divergent for all times longer past than a definite determinable epoch. The distribution of heat at such an epoch is essentially *initial*—that is to say, it cannot result from any previous condition of matter by natural processes. It is, then, well called an "*arbitrary* initial distribution of heat," in FOURIER's great mathematical poem, because it could only be realised by action of a power able to modify the laws of dead matter. In an article published about nineteen years ago in the "Cambridge Mathematical Journal,"† I gave the mathematical criterion for an essentially initial distribution; and in an inaugural essay, "De Motu Caloris per Terræ Corpus," read before the Faculty of the University of Glasgow in 1846, I suggested, as an application of these principles, that a perfectly complete geothermic survey would give us data for determining an initial epoch in the problem of terrestrial conduction. At the meeting of the British Association in Glasgow in 1855, I urged that special geothermic surveys should be made for the purpose of estimating absolute dates in geology, and I pointed out some cases, especially that of the salt-spring borings at Creuznach, in Rhenish Prussia, in which eruptions of basaltic rock seem to leave traces of their igneous origin in residual heat.‡ I hope this suggestion may yet be taken up, and may prove to some extent useful; but the disturbing influences affecting underground temperature, as Professor PHILLIPS has well shown in a recent inaugural address to the Geological Society, are too great to allow us to expect any very precise or satisfactory results.

5. The chief object of the present communication is to estimate from the known general increase of temperature in the earth downwards, the date of the first establishment of that "consistentior status," which, according to LEIBNITZ's theory, is the initial date of all geological history.

6. In all parts of the world in which the earth's crust has been examined, at sufficiently great depths to escape large influence of the irregular and of the annual variations of the superficial temperature, a gradually increasing temperature has been found in going deeper. The rate of augmentation (estimated at only $\frac{1}{110}$th of a degree, Fahr., in some localities, and as much as $\frac{1}{15}$th of a degree in others, per foot of descent) has not been observed in a sufficient number

* Proceedings Royal Soc. Edin., Feb. 1852. "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy." Also, "On the Restoration of Energy in an Unequally Heated Space," Phil. Mag., 1853, first half year.

† Feb. 1844.—"Note on Certain Points in the Theory of Heat."

‡ See British Association Report of 1855 (Glasgow) Meeting.

of places to establish any fair average estimate for the upper crust of the whole earth. But $\frac{1}{50}$th is commonly accepted as a rough mean; or, in other words, it is assumed as a result of observation, that there is, on the whole, about 1° Fahr. of elevation of temperature per 50 British feet of descent.

7. The fact that the temperature increases with the depth implies a continual loss of heat from the interior, by conduction outwards through or into the upper crust. Hence, since the upper crust does not become hotter from year to year, there must be a secular loss of heat from the whole earth. It is possible that no cooling may result from this loss of heat, but only an exhaustion of potential energy, which in this case could scarcely be other than chemical affinity between substances forming part of the earth's mass. But it is certain that either the earth is becoming on the whole cooler from age to age, or the heat conducted out is generated in the interior by temporary dynamical (that is, in this case, chemical) action. To suppose, as LYELL, adopting the chemical hypothesis, has done,* that the substances, combining together, may be again separated electrolytically by thermo-electric currents, due to the heat generated by their combination, and thus the chemical action and its heat continued in an endless cycle, violates the principles of natural philosophy in exactly the same manner, and to the same degree, as to believe that a clock constructed with a self-winding movement may fulfil the expectations of its ingenious inventor by going for ever.

8. It must indeed be admitted that many geological writers of the "Uniformitarian" school, who in other respects have taken a profoundly philosophical view of their subject, have argued in a most fallacious manner against hypotheses of violent action in past ages. If they had contented themselves with showing that many existing appearances, although suggestive of extreme violence and sudden change, may have been brought about by long-continued action, or by paroxysms not more intense than some of which we have experience within the periods of human history, their position might have been unassailable; and certainly could not have been assailed except by a detailed discussion of their facts. It would be a very wonderful, but not an absolutely incredible result, that volcanic action has never been more violent on the whole than during the last two or three centuries; but it is as certain that there is now less volcanic energy in the whole earth than there was a thousand years ago, as it is that there is less gunpowder in a "Monitor" after she has been seen to discharge shot and shell, whether at a nearly equable rate or not, for five hours without receiving fresh supplies, than there was at the beginning of the action. Yet this truth has been ignored or denied by many of the leading geologists of the present day, because they believe that the facts within their province do not demonstrate greater violence in ancient changes of the earth's surface, or do demonstrate a nearly equable action in all periods.

* Principles of Geology.

9. The chemical hypothesis to account for underground heat might be regarded as not improbable, if it was only in isolated localities that the temperature was found to increase with the depth; and, indeed, it can scarcely be doubted that chemical action exercises an appreciable influence (possibly negative, however) on the action of volcanoes; but that there is slow uniform "combustion," "eremacausis," or chemical combination of any kind going on, at some great unknown depth under the surface everywhere, and creeping inwards gradually as the chemical affinities in layer after layer are successively saturated, seems extremely improbable, although it cannot be pronounced to be absolutely impossible, or contrary to all analogies in nature. The less hypothetical view, however, that the earth is merely a warm chemically inert body cooling, is clearly to be preferred in the present state of science.

10. POISSON'S celebrated hypothesis, that the present underground heat is due to a passage, at some former period, of the solar system through hotter stellar regions, cannot provide the circumstances required for a palæontology continuous through that epoch of external heat. For from a mean of values of the conductivity, in terms of the thermal capacity of unit volume, of the earth's crust, in three different localities near Edinburgh, which I have deduced from the observations on underground temperature instituted by Principal FORBES there, I find that if the supposed transit through a hotter region of space took place between 1250 and 5000 years ago, the temperature of that supposed region must have been from 25° to 50° Fahr. above the present mean temperature of the earth's surface, to account for the present general rate of under-ground increase of temperature, taken as 1° Fahr. in 50 feet downwards. Human history negatives this supposition. Again, geologists and astronomers will, I presume, admit that the earth cannot, 20,000 years ago, have been in a region of space 100° Fahr. warmer than its present surface. But if the transition from a hot region to a cool region supposed by POISSON took place more than 20,000 years ago, the excess of temperature must have been more than 100° Fahr., and must therefore have destroyed animal and vegetable life. Hence, the farther back and the hotter we can suppose POISSON'S hot region, the better for the geologists who require the longest periods; but the best for their view is LEIBNITZ'S theory, which simply supposes the earth to have been at one time an incandescent liquid, without explaining how it got into that state. If we suppose the temperature of melting rock to be about 10,000° Fahr. (an extremely high estimate), the consolidation may have taken place 200,000,000 years ago. Or, if we suppose the temperature of melting rock to be 7000° Fahr. (which is more nearly what it is generally assumed to be), we may suppose the consolidation to have taken place 98,000,000 years ago.

11. These estimates are founded on the Fourier solution demonstrated below. The greatest variation we have to make on them, to take into account the differences in the ratios of conductivities to specific heats of the three Edinburgh rocks,

is to reduce them to nearly half, or to increase them by rather more than half. A reduction of the Greenwich underground observations recently communicated to me by Professor EVERETT of Windsor, Nova Scotia, gives for the Greenwich rocks a quality intermediate between those of the Edinburgh rocks. But we are very ignorant as to the effects of high temperatures in altering the conductivities and specific heats of rocks, and as to their latent heat of fusion. We must, therefore, allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation cannot have taken place less than 20,000,000 years ago, or we should have more underground heat than we actually have, nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that LEIBNITZ's epoch of "emergence" of the "consistentior status" was probably between those dates.

12. The mathematical theory on which these estimates are founded is very simple, being in fact merely an application of one of FOURIER's elementary solutions to the problem of finding at any time the rate of variation of temperature from point to point, and the actual temperature at any point, in a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two different constant values on the two sides of a certain infinite plane. The solution for the two required elements is as follows:—

$$\frac{dv}{dx}=\frac{V}{\sqrt{\pi\kappa t}}\epsilon^{-\frac{x^2}{4\kappa t}}$$

$$v=v_0+\frac{2V}{\sqrt{\pi}}\int_0^{\frac{x}{2\sqrt{\kappa t}}} dz\epsilon^{-z^2}$$

where κ denotes the conductivity of the solid, measured in terms of the thermal capacity of the unit of bulk;

V, half the difference of the two initial temperatures;
v_0, their arithmetical mean;
t, the time;
x, the distance of any point from the middle plane;
v, the temperature of the point x at time t;

and, consequently (according to the notation of the differential calculus), $\frac{dv}{dx}$ the rate of variation of the temperature per unit of length perpendicular to the isothermal planes.

13. To demonstrate this solution, it is sufficient to verify—(1), That the expression for v fulfils the partial differential equation,

$$\frac{dv}{dt}=\kappa\frac{d^2v}{dx^2},$$

FOURIER's equation for the "linear conduction of heat;" (2) That when $t = 0$,

the expression for v becomes $v_0 + V$ for all positive, and $v_0 - V$ for all negative values of x; and (3), That the expression for $\frac{dv}{dx}$ is the differential co-efficient with reference to x, of the expression for v. The propositions (1) and (3) are proved directly by differentiation. To prove (2), we have, when $t = 0$, and x positive,

$$v = v_0 + \frac{2V}{\sqrt{\pi}} \int_0^{\infty} dz \epsilon^{-z^2}$$

or according to the known value, $\frac{1}{2}\sqrt{\pi}$, of the definite integral $\int_0^{\infty} dz \epsilon^{-z^2}$,

$$v = v_0 + V;$$

and for all values of t, the second term has equal positive and negative values for equal positive and negative values of x, so that when $t = 0$ and x negative,

$$v = v_0 - V.$$

The admirable analysis by which FOURIER arrived at solutions including this, forms a most interesting and important mathematical study. It is to be found in his "Théorie Analytique de la Chaleur." Paris, 1822.

14. The accompanying diagram represents, by two curves, the preceding expressions for $\frac{dv}{dx}$ and v respectively.

15. The solution thus expressed and illustrated applies, for a certain time, without sensible error, to the case of a solid sphere, primitively heated to a uniform temperature, and suddenly exposed to any superficial action, which for ever after keeps the surface at some other constant temperature. If, for instance, the case considered is that of a globe 8000 miles diameter of solid rock, the solution will apply with scarcely sensible error for more than 1000 millions of years. For, if the rock be of a certain average quality as to conductivity and specific heat, the value of κ, as I have shown in a previous communication to the Royal Society,* will be 400, to unit of length a British foot and unit of time a year; and the equation expressing the solution becomes

$$\frac{dv}{dx} = \frac{V}{35{\cdot}4} \cdot \frac{1}{t^{\frac{1}{2}}} \cdot \epsilon^{-\frac{x^2}{1600\,t}};$$

and if we give t the value 1,000,000,000, or anything less, the exponential factor becomes less than $\epsilon^{-5{\cdot}6}$ (which being equal to about $\frac{1}{270}$, may be regarded as insensible), when x exceeds 3,000,000 feet, or 568 miles. That is to say, during the first 1000 million years the variation of temperature does not become sensible at depths exceeding 568 miles, and is therefore confined to so thin a crust, that the influence of curvature may be neglected.

* On the Periodical Variations of Underground Temperature. Trans. Roy. Soc. Edin. March 1860.

16. If, now, we suppose the time to be 100 million years from the commencement of the variation, the equation becomes

$$\frac{dv}{dx}=\frac{V}{354000}\epsilon^{-\frac{x^2}{160000000000}}$$

The diagram, therefore, shows the variation of temperature which would now exist in the earth, if, its whole mass being first solid and at one temperature 100 million years ago, the temperature of its surface had been everywhere suddenly lowered by V degrees, and kept permanently at this lower temperature: the scales used being as follows :—

(1) For depth below the surface,—scale along OX, 10 quarter inches, or a, represents 400,000 feet.

(2) For rate of increase of temperature per foot of depth,—scale of ordinates parallel to OY, 10 half inches, or b, represents $\frac{1}{354000}$ of V per foot. If, for example, $V=7000°$, this scale will be such that 10 half inches, or b, represents $\frac{1}{50{\cdot}6}$ of a degree per foot.

(3) For excess of temperature,—scale of ordinates parallel to OY, 10 half inches, or b, represents $\frac{V}{\frac{1}{2}\sqrt{\pi}}$, or 7900°, if V = 7000°.

Thus the rate of increase of temperature from the surface downwards would be sensibly $\frac{1}{51}$ of a degree per foot for the first 100,000 feet or so. Below that depth the rate of increase per foot would begin to diminish sensibly. At 400,000 feet it would have diminished to about $\frac{1}{141}$ of a degree per foot. At 800,000 feet it would have diminished to less than $\frac{1}{50}$ of its initial value,—that is to say, to less than $\frac{1}{2550}$ of a degree per foot; and so on, rapidly diminishing, as shown in the curve. Such is, on the whole, the most probable representation of the earth's present temperature, at depths of from 100 feet, where the annual variations cease to be sensible, to 100 miles; below which the whole mass, or all except a nucleus cool from the beginning, is (whether liquid or solid) probably at, or very nearly at, the proper melting temperature for the pressure at each depth.

17. The theory indicated above throws light on the question so often discussed—Can terrestrial heat have influenced climate through long geological periods? and allows us to answer it very decidedly in the negative. There would be an increment of temperature at the rate of 2° Fahr. per foot downwards near the surface, 10,000 years after the beginning of the cooling, in the case we have supposed. The radiation from earth and atmosphere into space (of which we have yet no satisfactory absolute measurement) would almost certainly be so rapid in the earth's actual circumstances, as not to allow a rate of increase of 2° Fahr. per foot underground to augment sensibly the temperature of the sur-

face; and hence I infer that the general climate cannot be sensibly affected by conducted heat at any time more than 10,000 years after the commencement of superficial solidification. No doubt, however, in particular places there might be an elevation of temperature by thermal springs, or by eruptions of melted lava, and everywhere vegetation would, for the first 3,000,000 or 4,000,000 years, if it existed so soon after the epoch of consolidation, be influenced by the sensibly higher temperature met with by roots extending a foot or more below the surface.

18. Whatever the amount of such effects is at any one time, it would go on diminishing according to the inverse proportion of the square roots of the times from the initial epoch. Thus, if at 10,000 years we have 2° per foot of increment below ground,

At	40,000	years we should have	1°	per foot.
„	160,000	„ „	$\frac{1}{2}$°	„
„	4,000,000	„ „	$\frac{1}{10}$°	„
„	100,000,000	„ „	$\frac{1}{50}$°	„

It is therefore probable that for the last 96,000,000 years the rate of increase of temperature under ground has gradually diminished from about $\frac{1}{10}$th to about $\frac{1}{50}$th of a degree Fahrenheit per foot, and that the thickness of the crust through which any stated degree of cooling has been experienced has gradually increased in that period from $\frac{1}{5}$th of what it is now to what it is. Is not this, on the whole, in harmony with geological evidence, rightly interpreted? Do not the vast masses of basalt, the general appearances of mountain-ranges, the violent distortions and fractures of strata, *the great prevalence of metamorphic action* (which must have taken place at depths of not many miles, if so much), all agree in demonstrating that the rate of increase of temperature downwards must have been much more rapid, and in rendering it probable that volcanic energy, earthquake shocks, and every kind of so-called plutonic action, have been, on the whole, more abundantly and violently operative in geological antiquity than in the present age?

19. But it may be objected to this application of mathematical theory—(1), That the earth was once all melted, or at least melted all round its surface, and cannot possibly, or rather cannot with any probability, be supposed to have been ever a uniformly heated solid, 7000° warmer than our present surface temperature, as assumed in the mathematical problem; and (2), No natural action could possibly produce at one instant, and maintain for ever after, a seven thousand degrees' lowering of the surface temperature. Taking the second objection first, I answer it by saying, what I think cannot be denied, that a large mass of melted rock, exposed freely to our air and sky, will, after it once becomes crusted over, present in a few hours, or a few days, or at the most a few weeks, a surface so cool that it can be walked over with impunity. Hence, after 10,000 years, or, indeed, I may say after a single year, its condition will be sensibly the same as if the actual lowering of temperature experienced by the surface had been produced in an

instant and maintained constant ever after. I answer the first objection by saying, that if experimenters will find the latent heat of fusion, and the variations of conductivity and specific heat of the earth's crust up to its melting point, it will be easy to modify the solution given above, so as to make it applicable to the case of a liquid globe gradually solidifying from without inwards, in consequence of heat conducted through the solid crust to a cold external medium. In the meantime, we can see that this modification will not make any considerable change in the resulting temperature of any point in the crust, unless the latent heat parted with on solidification proves, contrary to what we may expect from analogy, to be considerable in comparison with the heat that an equal mass of the solid yields in cooling from the temperature of solidification to the superficial temperature. But, what is more to the purpose, it is to be remarked that the objection, plausible as it appears, is altogether fallacious, and that the problem solved above corresponds much more closely, in all probability, with the actual history of the earth, than does the modified problem suggested by the objection. The earth, although once all melted, or melted all round its surface, did, in all probability, really become a solid at its melting temperature all through, or all through the outer layer, which had been melted; and not until the solidification was thus complete, or nearly so, did the surface begin to cool. That this is the true view can scarcely be doubted, when the following arguments are considered.

20. In the first place, we shall assume that at one time the earth consisted of a solid nucleus, covered all round with a very deep ocean of melted rocks, and left to cool by radiation into space. This is the condition that would supervene, on a cold body much smaller than the present earth meeting a great number of cool bodies still smaller than itself, and is therefore in accordance with what we may regard as a probable hypothesis regarding the earth's antecedents. It includes, as a particular case, the commoner supposition, that the earth was once melted throughout, a condition which might result from the collision of two nearly equal masses. But the evidence which has convinced most geologists that the earth had a fiery beginning, goes but a very small depth below the surface, and affords us absolutely no means of distinguishing between the actual phenomena, and those which would have resulted from either an entire globe of liquid rock, or a cool solid nucleus covered with liquid to any depth exceeding 50 or 100 miles. Hence, irrespectively of any hypothesis as to antecedents from which the earth's initial fiery condition may have followed by natural causes, and simply assuming, as rendered probable by geological evidence, that there was at one time melted rock all over the surface, we need not assume the depth of this lava ocean to have been more than 50 or 100 miles; although we need not exclude the supposition of any greater depth, or of an entire globe of liquid.

21. In the process of refrigeration, the fluid must (as I have remarked regarding the sun, in a recent article in "Macmillan's Magazine,"* and regarding the earth's atmosphere, in a communication to the Literary and Philosophical Society of Manchester†) be brought by convection, to fulfil a definite law of distribution of temperature which I have called "convective equilibrium of temperature." That is to say, the temperatures at different parts in the interior must differ according to the different pressures by the difference of temperatures which any one portion of the liquid would present, if given at the temperature and pressure of any part, and then subjected to variation of pressure, but prevented from losing or gaining heat. The reason for this is the extreme slowness of true thermal conduction; and the consequently preponderating influence of great currents throughout a continuous fluid mass, in determining the distribution of temperature through the whole.

22. The thermo-dynamic law connecting temperature and pressure in a fluid mass, not allowed to lose or gain heat, investigated theoretically, and experimentally verified in the cases of air and water, by Dr JOULE and myself,‡ shows, therefore, that the temperature in the liquid will increase from the surface downwards, if, as is most probably the case, the liquid contracts in cooling. On the other hand, if the liquid, like water near its freezing-point, expanded in cooling, the temperature, according to the convective and thermo-dynamic laws just stated (§§ 21, 22), would actually be lower at great depths than near the surface, even although the liquid is cooling from the surface; but there would be a very thin superficial layer of lighter and cooler liquid, losing heat by true conduction, until solidification at the surface would commence.

23. Again, according to the thermo-dynamic law of freezing, investigated by my brother,§ Professor JAMES THOMSON, and verified by myself experimentally for water,‖ the temperature of solidification will, at great depths, because of the great pressure, be higher there than at the surface if the fluid contracts, or lower than at the surface if it expands, in becoming solid.

24. How the temperature of solidification, for any pressure, may be related to the corresponding temperature of fluid convective equilibrium, it is impossible to say, without knowledge, which we do not yet possess, regarding the expansion

* March 1862.

† "Proceedings," Jan. 1862. "On the Convective Equilibrium of Temperature in the Atmosphere."

‡ JOULE, "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," Phil. Mag. about 1844. THOMSON, "On a Method for Determining Experimentally the Heat evolved by the Compression of Air; Dynamical Theory of Heat, Part IV.," Trans. R. S. E., Session 1850–51; and reprinted, Phil. Mag. JOULE and THOMSON, "On the Thermal Effects of Fluids in Motion," Trans. R. S. Lond., June 1853 and June 1854. JOULE and THOMSON, "On the Alterations of Temperature accompanying Changes of Pressure in Fluids," Proceedings R. S. Lond., June 1857.

§ "Theoretical Considerations Regarding the Effect of Pressure in lowering the Freezing-Point of Water," Trans. R. S. E., Jan. 1849.

‖ Proceedings R. S. E., Session 1849–50.

with heat, and the specific heat of the fluid, and the change of volume, and the latent heat developed in the transition from fluid to solid.

25. For instance, supposing, as is most probably true, both that the liquid contracts in cooling towards its freezing-point, and that it contracts in freezing, we cannot tell, without definite numerical data regarding those elements, whether the elevation of the temperature of solidification, or of the actual temperature of a portion of the fluid given just above its freezing-point, produced by a given application of pressure, is the greater. If the former is greater than the latter, solidification would commence at the bottom, or at the centre, if there is no solid nucleus to begin with, and would proceed outwards; and there could be no complete permanent incrustation all round the surface till the whole globe is solid, with, possibly, the exception of irregular, comparatively small spaces of liquid.

26. If, on the contrary, the elevation of temperature, produced by an application of pressure to a given portion of the fluid, is greater than the elevation of the freezing temperature produced by the same amount of pressure, the superficial layer of the fluid would be the first to reach its freezing-point, and the first actually to freeze.

27. But if, according to the second supposition of § 22, the liquid expanded in cooling near its freezing-point, the solid would probably likewise be of less specific gravity than the liquid at its freezing-point. Hence the surface would crust over permanently with a crust of solid, constantly increasing inwards by the freezing of the interior fluid in consequence of heat conducted out through the crust. The condition most commonly assumed by geologists would thus be produced.

28. But BISCHOF'S experiments, upon the validity of which, so far as I am aware, no doubt has ever been thrown, show that melted granite, slate, and trachyte, all contract by something about 20 per cent. in freezing. We ought, indeed, to have more experiments on this most important point, both to verify BISCHOF'S results on rocks, and to learn how the case is with iron and other unoxydised metals. In the meantime we must consider it as probable that the melted substance of the earth did really contract by a very considerable amount in becoming solid.

29. Hence, if according to any relations whatever among the complicated physical circumstances concerned, freezing did really commence at the surface, either all round or in any part, before the whole globe had become solid, the solidified superficial layer must have broken up and sunk to the bottom, or to the centre, before it could have attained a sufficient thickness to rest stably on the lighter liquid below. It is quite clear, indeed, that if at any time the earth were in the condition of a thin solid shell of, let us suppose 50 feet or 100 feet thick of granite, enclosing a continuous melted mass of 20 per cent. less specific

gravity in its upper parts, where the pressure is small, this condition cannot have lasted many minutes. The rigidity of a solid shell of superficial extent so vast in comparison with its thickness, must be as nothing, and the slightest disturbance would cause some part to bend down, crack, and allow the liquid to run out over the whole solid. The crust itself would in consequence become shattered into fragments, which must all sink to the bottom, or to meet in the centre and form a nucleus there if there is none to begin with.

30. It is, however, scarcely possible, that any such continuous crust can ever have formed all over the melted surface at one time, and afterwards have fallen in. The mode of solidification conjectured in § 25, seems on the whole the most consistent with what we know of the physical properties of the matter concerned. So far as regards the result, it agrees, I believe, with the view adopted as the most probable by Mr HOPKINS.* But whether from the condition being rather that described in § 26, which seems also possible, for the whole or for some parts of the heterogeneous substance of the earth, or from the viscidity as of mortar, which necessarily supervenes in a melted fluid, composed of ingredients becoming, as the whole cools, separated by crystallising at different temperatures before the solidification is perfect, and which we actually see in lava from modern volcanoes; it is probable that when the whole globe, or some very thick superficial layer of it, still liquid or viscid, has cooled down to near its temperature of perfect solidification, incrustation at the surface must commence.

31. It is probable that crust may thus form over wide extents of surface, and may be temporarily buoyed up by the vesicular character it may have retained from the ebullition of the liquid in some places, or, at all events, it may be held up by the viscidity of the liquid; until it has acquired some considerable thickness sufficient to allow gravity to manifest its claim, and sink the heavier solid below the lighter liquid. This process must go on until the sunk portions of crust build up from the bottom a sufficiently close ribbed solid skeleton or frame, to allow fresh incrustations to remain bridging across the now small areas of lava pools or lakes.

32. In the honey-combed solid and liquid mass thus formed, there must be a continual tendency for the liquid, in consequence of its less specific gravity, to work its way up; whether by masses of solid falling from the roofs of vesicles or tunnels, and causing earthquake shocks, or by the roof breaking quite through when very thin, so as to cause two such hollows to unite, or the liquid of any of them to flow out freely over the outer surface of the earth; or by gradual subsidence of the solid, owing to the thermo-dynamic melting, which portions of it, under intense stress, must experience, according to views recently published by my brother, Pro-

* See his Report on "Earthquakes and Volcanic Action." British Association Report for 1847.

fessor JAMES THOMSON.* The results which must follow from this tendency seem sufficiently great and various to account for all that we see at present, and all that we learn from geological investigation, of earthquakes, of upheavals and subsidences of solid, and of eruptions of melted rock.

33. These conclusions, drawn solely from a consideration of the necessary order of cooling and consolidation, according to BISCHOF's result as to the relative specific gravities of solid and of melted rock, are in perfect accordance with what I have recently demonstrated† regarding the present condition of the earth's interior,—that it is not, as commonly supposed, all liquid within a thin solid crust of from 30 to 100 miles thick, but that it is on the whole more rigid certainly than a continuous solid globe of glass of the same diameter, and probably than one of steel.

* Proceedings of the Royal Society of London 1861, "On Crystallization and Liquefaction as influenced by Stresses tending to Change of Form in Crystals."

† In a paper "On the Rigidity of the Earth," communicated to the Royal Society a few days ago. April 1862.

29 NO 62

INCREASE OF TEMPERATURE DOWNWARDS IN THE EARTH

$ON = x$

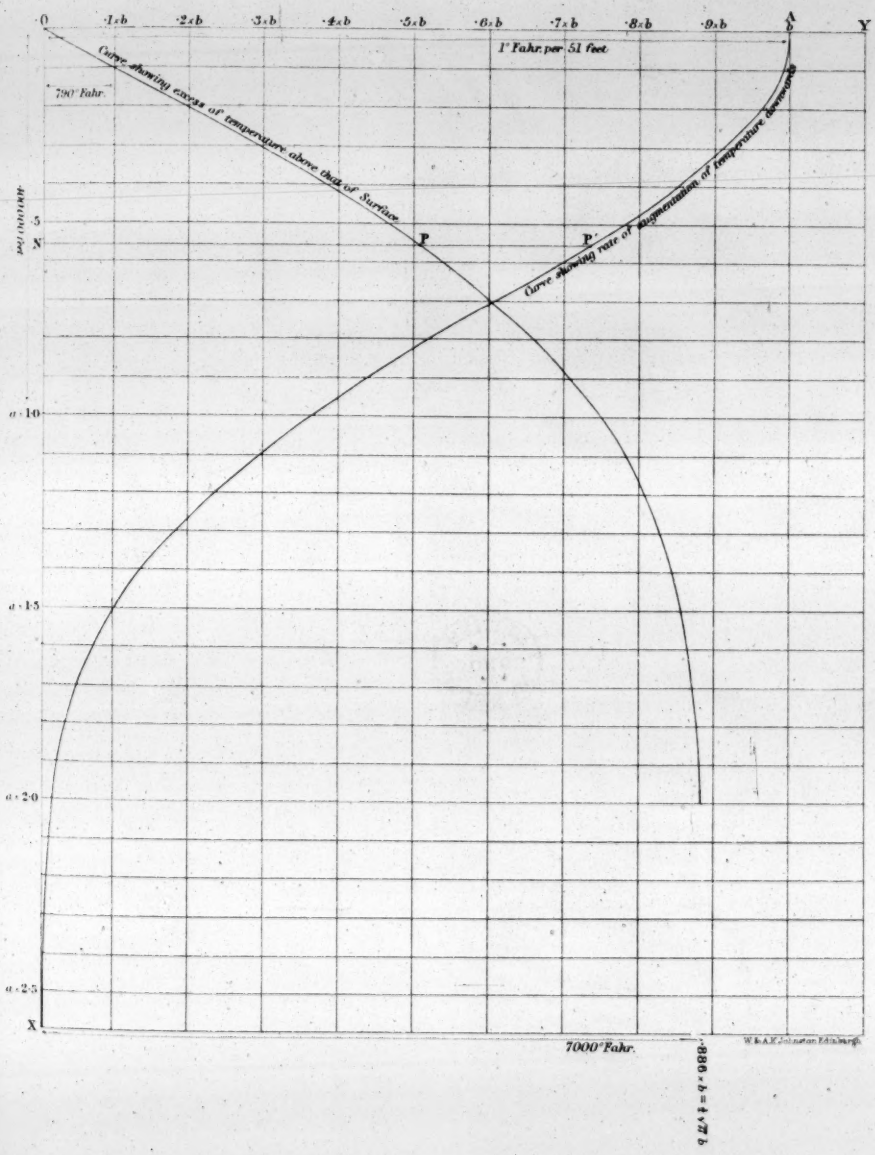
$NP' = b\epsilon^{-\frac{x^2}{a^2}} = y'$

$NP = \text{area } ONP'A + a = \frac{1}{a}\int_o^x y'\,dx$

$a = 2\sqrt{kt}$

$\frac{dv}{dx} = \frac{V}{a}\cdot\frac{NP'}{b\sqrt{\pi}}$

$v - v_o = V\cdot\frac{NP}{b\sqrt{\pi}}$



Page

ART. IX.—*On the Deflection of the Plummet due to Solar and Lunar Attraction.* By EDWARD SANG, Esq. (With a Plate, V.), ... 89

X.—*On the Existence of Acari between the Laminæ of Mica in Optical Contact.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S. (With a Plate, VI.), . . . 95

XI.—*On Certain Vegetable and Mineral Formations in Calcareous Spar.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S. (With a Plate, VII.), ... 97

XII.—*Memoir of the Life and Writings of Robert Whytt, M.D., Professor of Medicine in the University of Edinburgh from* 1747 *to* 1766. By WILLIAM SELLER, M.D., F.R.S.E., Fellow of the Royal College of Physicians of Edinburgh, ... 99

XIII.—*Experimental Inquiry into the Laws of the Conduction of Heat in Bars, and into the Conducting Power of Wrought Iron.* By JAMES D. FORBES, LL.D., D.C.L., F.R.S., V.P.R.S. Ed., Corresponding Member of the Institute of France, Principal of the United College of St Salvator and St Leonard, St Andrews, ... 133

XIV.—*On the Density of Steam.* By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS. Lond. and Edin., &c., ... 147

XV.—*On the Secular Cooling of the Earth.* By Professor WILLIAM THOMSON, LL.D., F.R.S., F.R.S.E. (With a Plate, VIII.), ... 157

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			„ Part 3.	1 5 0	1 1 0

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TRANSACTIONS

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VOL. XXIII. PART II.—FOR THE SESSION 1862–63.

CONTENTS.

Page

ART. XVI.—*On the Representative Relationships of the Fixed and Free Tunicata, regarded as two Sub-Classes of equivalent value; with some General Remarks on their Morphology.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (Plate IX.), ... 171

XVII.—*On the Zoological Characters of the Living* Clio caudata, *as compared with those of* Clio borealis *given in Systematic Works.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (Plate, IX. fig. 3), ... 185

XVIII.—*Notes on the Anatomy of the Genus Firola.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (Plate IX. fig. 4), ... 189

XIX.—*On the Structure and Optical Phenomena of Ancient Decomposed Glass.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., &c. (Plates X., XI.), ... 193

XX.—*On the Polarisation of Light by Rough and White Surfaces.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., &c., ... 205

XXI.—*Observations on the Polarisation of the Atmosphere, made at St Andrews in* 1841, 1842, 1843, 1844, *and* 1845. By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., &c., (Plate XII.), ... 211

[*For remainder of Contents, see last page of Cover.*]

XVI.—*On the Representative Relationships of the Fixed and Free Tunicata, regarded as two Subclasses of equivalent value; with some General Remarks on their Morphology.* By JOHN DENIS MACDONALD, R.N., F.R.S, Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (Plate IX.)

(Read 15th December 1862.)

My first precise views of the structure of the Tunicata were formed by the perusal of Mr HUXLEY's masterly papers dealing with the anatomy of *Salpa*, *Pyrosoma*, *Doliolum*, and *Appendicularia*, in the Phil. Trans. for 1851, Part II.; and I have since had abundant opportunity of verifying all the important facts, made known by that original observer, in the papers to which I have alluded.

Having thus acknowledged my guide in this field of research, I can scarcely claim much originality as relates to pure anatomy; but I hope that the method here adopted, in opening up this interesting subject, will be found in keeping with nature, as it is the result of much study and practical investigation.

I must first beg the question, and next endeavour to support it, that the class *Tunicata* may be conveniently divided into two subclasses—viz., the fixed or stationary, and the free or locomotive. The latter, from their habit of life, are also commonly denominated *Pelagic;* while the former, by general consent, chiefly following the suggestions of M. MILNE-EDWARDS, have been divided into the *Simple*, the *Social*, and the *Compound*, as given in the following table:—

Tunicata.

I. Fixed or stationary.	
1. Solitary, or simply segregate.	
a. Sessile (recumbent or erect), or pedunculate, ...	*Simple.*
2. Organically blended in communities.	
a. Sessile or pedunculated on a common axis, ...	*Social.*
b. Immersed in a common test substance, ...	*Compound.*
II. Free or locomotive, ...	*Pelagic.*

Of the fixed Tunicata, it would appear that *Pelonaia* and *Chelyosoma** present characters which at once distinguish them from all the other members of the subclass. Thus, in the genera named, the branchial membrane seems to be closely adherent to the subjacent textures, without forming a distinct sac,

* We are much in want of more accurate information respecting these genera. Mr HUXLEY remarks, *loc. cit.* 588, "In *Pelonaia*, the hypopharyngeal band has disappeared. It is a *Salpa* in which the oral and cloacal orifices have approximated, while the 'gill' has become obliterated;" and in a note at the bottom of the page he says, "*Chelyosoma* would appear to resemble *Pelonaia* in the absence of any distinct branchial sac; but ESCHRICHT's figures are not very clear."

though finely areolated and thrown into parallel folds; whereas, in all the remaining fixed Tunicata, *a branchial sac may be demonstrated, the respiratory slits or meshes being in general longitudinal, and disposed in many transverse series.* On the other hand, the respiratory system in the free or Pelagic section presents no less than four distinct types, as occurring respectively in the genera *Pyrosoma*, *Doliolum*, *Salpa*, and *Appendicularia*. These may be defined as follows:—

Branchial membrane sac-like, with transverse slits in single longitudinal series, strengthened by longitudinal non-ciliated bars. Apertures terminal or subterminal—PYROSOMA.

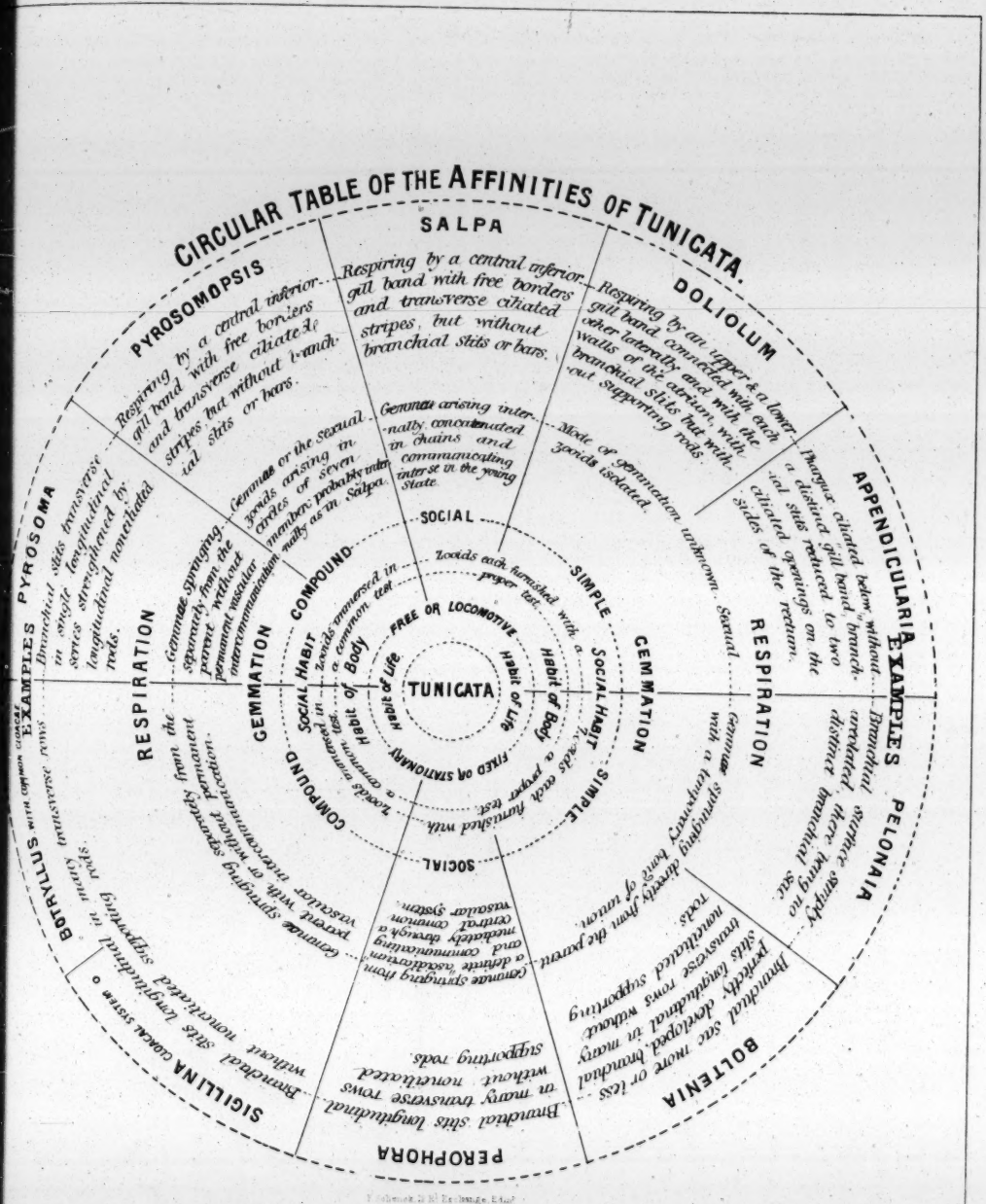
Respiring by an upper and a lower "gill-band," connected with each other laterally, and with the walls of the atrium; having branchial slits, but no supporting longitudinal bars. Apertures terminal—DOLIOLUM.

Respiring by a central inferior gill-band, with free borders and transverse ciliated stripes, but without slits or bars. Apertures terminal or subterminal—SALPA, &c.

Pharynx ciliated below without a distinct "gill-band;" branchial slits reduced to two ciliated openings on the sides of the rectum. Apertures approximated hæmally—APPENDICULARIA.

As far as the respiratory system is concerned, therefore, the fixed Tunicata exhibit at least two well-marked types, and the Pelagic group four, which are equally distinct, and, as I conceive, of equal importance, demanding fair consideration in systematic arrangement. Moreover, I am quite satisfied that there are very striking representative relationships existing between the fixed and free Tunicata; and in order to exhibit these the more clearly, I have drawn up the annexed circular scheme or table, in which it will be seen that each subclass has its simple, social, and compound groups, and their mutual representatives may be at once recognised. Thus, *Appendicularia* represents the equally curious genus *Pelonaia*, *Doliolum* the remaining simple Tunicata, *Salpa* the social, and *Pyrosoma*, the compound group, but, in particular, the *Botryllians*. Any one accustomed to draw comparisons and analogies will readily perceive that there is something more than simple coincidence in all this, particularly as the characters employed are so comprehensive, bearing successively on habit of life, habit of body, social habit, mode of gemmation, and respiration. It would be quite as easy to draw up two circles as one, or the characters might be thrown into rays instead of circles, but the result in all cases would be virtually the same.

I always pay respect to the maxim, that "characters can only be taken as natural when they have been proved to be so;" and I know also that groups must be found, before trustworthy characters can be chosen to effect a classification. When we find *Nerites* breathing air, and feeding on the green leaves of the


CIRCULAR TABLE OF THE AFFINITIES OF TUNICATA.
SALPA
Respiring by a central inferior gill band with free borders and transverse ciliated stripes, but without branchial slits or bars.
Gemmae arising internally concatenated in chains and commencing interest in the young state.
Mode of gemmation unknown. Zooids isolated.
PYROSOMOPSIS
Respiring by a central inferior gill band with free borders and transverse ciliated stripes but without branchial slits or bars.
Gemmae or the sexual zooids arising in cycles of seven, probably inter-communicating as in Salpa.
DOLIOLUM
Respiring by an upper & a lower gill band connected with each other laterally and with the walls of the atrium, with branchial slits but without supporting rods.
Thoracic ciliated gill band, branchial slits reduced to two ciliated openings on the sides of the rectum.
Sexual.
TUNICATA
HABIT of Life
FREE OR LOCOMOTIVE
FIXED OR STATIONARY
Habit of Body
SOCIAL
COMPOUND
SIMPLE
SOCIAL HABIT
Zooids immersed in a common test
Zooids each furnished with a proper test
RESPIRATION
GEMMATION
EXAMPLES PYROSOMA
Branchial slits transverse in single longitudinal series strengthened by longitudinal nonciliated rods.
Gemmae springing separately from the parent without permanent vascular intercommunication.
BOTRYLLUS with common cloacal system
Branchial slits transverse in many rows.
Gemmae springing from the parent with permanent vascular intercommunication.
SIGILLINA
Branchial slits longitudinal in many rows without nonciliated supporting rods.
Gemmae springing from a definite tuberculum and communicating by central common vascular system.
PERophora
Branchial slits longitudinal in many transverse rows without nonciliated supporting rods.
Gemmae springing from a definite tuberculum and communicating by central common vascular system.
BOLTENIA
Branchial sac more or less perfectly developed, branchial slits longitudinal in many transverse rows without nonciliated supporting rods.
Gemmae with a temporary band of tunic.
PELONAIA
Branchial sac rather small, without nonciliated supporting rods.
Gemmae springing directly from the parent.
APPENDICULARIA EXAMPLES

forest, *Nerites* breathing in the mountain streams, *Nerites* in brackish waters, *Nerites* on the beach in fellowship with *Littorina*, and in the sea itself, one would say that habit of life would afford anything but a natural character. Nor indeed would it in the case given, and in many others that might be adduced; but it certainly affords us the simplest and most natural primary division of *Tunicata*, and the other characters adopted in the table appear to answer their purpose equally well.

The idea has been too commonly entertained, that the Pelagic Tunicata, so called, compose a group only commensurate with the compound, the social, or the simple, taken separately, but I trust that a candid analysis of the preceding table will serve to free the judgment from an accustomed bias in this particular, and show that the fixed and the free Tunicata form two subclasses of at least nearly equal value in a zoological point of view; and I almost imagine, though myself affected by the prejudice to which I have alluded, that the balance is rather in favour of the Pelagians.

Though we may readily, as by a kind of empiricism, recognise a difference between the *simple*, *social*, and *compound Tunicata*, it is by no means easy to give them an intelligible definition. Indeed, I can safely say, as a student of the Tunicata, that I have long known more of the existence of a difference than of its precise nature, and this can scarcely be arrived at by the study of books alone. Indeed, the whole subject is even now shadowy and ill defined, notwithstanding the great light that has been shed upon it by the labours of Savigny, Macleay, Fleming, Milne-Edwards, and, in particular, Professor Huxley.

The term Compound is just as applicable to a tree of *Perophora*, as it is to one of *Sertularia*; but that term is restricted to another group, including forms that differ remarkably *inter se*, such as *Botryllus* and *Sigillina*, for example, their great characteristic being, as far as I can see, more or less complete immersion of the zooids in a common test, with or without vascular intercommunication. This immersion of the zooids is an important feature, as no common cloacal system can otherwise exist; but, inasmuch as it may occur without the formation of cloacæ, it links the *social* group with such compound Tunicaries as possess a common cloacal system. Furthermore, the branched and undermining form of this system, in several genera, indicates the passage to the Botryllian punctate, linear, or reticulate type, in connection with which latter, as a genetic character, several zooids are developed from a single ovum, as in *Pyrosoma*.

The process of gemmation, on the other hand, is much more energetic in the *social* than in either the *simple* or *compound* Ascidians. Thus, we scarcely ever find incipient buds springing from, or beyond others little farther advanced in the two latter, while such is the rule in the former group.

That the increase of the connecting substance, or "ascidiarium" of Huxley, proceeds *pari passu* with the gemmæ, and is, in fact, in advance of them in the

social Ascidians, as in the *Polyzoa*, can scarcely be denied; and upon it is impressed a limited law of growth or extension which is subservient to the forces determining the development of the zooids. Thus, while both progress in harmony, there are obvious indications of the co-existence of independent powers.

The primary fixed point, or, as it were, the potential germ of the ascidiarium, taken in the abstract, will be found to be very differently related to the zooids in the compound as compared with the social Ascidians. Thus, the point of attachment of a *Perophora* or *Chondrostachys*, for example, may be looked upon as homologous with the cloacal side of the apex, so called, in *Pyrosoma*, Plate IX. fig. 1 *a'*, or to the corresponding part in *Botryllus*, Plate IX. fig. 2 *a'*, the attached surface of the latter genus, Plate IX. fig. 2 *a*, being equivalent to the exterior side of the apex in the free *Pyrosoma*, and to the summit of the axis, produced in *Chondrostachys*, Plate IX. fig. 1 *a*, and depressed in *Diazona*, which latter genus appears to me to be more conformable with the social than with the compound group. The hæmal surface of the zooids, we therefore find, is turned in opposite directions in *Chondrostachys* and *Botryllus*,—viz., in the former case, towards the summit of the growing axis, and, in the latter, towards the margin of the encrusting common test, or, in other words, the zooids face outwards in one instance and inwards in the other. Though all the gemmæ augmenting the community are dorsal in both examples given, yet, in *Botryllus*, and especially in *Pyrosoma*, they are being continually thrown forward, so as ultimately to be in advance of the parent zooids, whereas in the social group, the anterior aspect of the primary zooid being turned towards the surface, upon which the rudimentary ascidiarium is fixed, the new gemmæ arise within extensions of the connecting substance in a truly retrograde direction,—*i. e.*, centripetally, or towards the summit of the central axis thus formed.

In the beautiful species of *Botrylloides*, of which there are many in the Australian seas, I have frequently observed a rear-rank of gemmæ advancing to usurp the place of their parents, which had for some time previously played their part at the fore; and indeed it is only in this way that the linear and reticulate cloacæ (along the sides of which two rows of zooids are at most to be found), can be produced.

The bell-shaped antroversion of the "ascidiarium" in *Pyrosoma* appears to be necessitated by the position of the "cyathozooid," so accurately described by Professor Huxley, and of the cloacal apertures of the four "ascidiozooids" surrounding it, as also by the peculiar mode of advance of the gemmæ, and the forward extension of these palliovascular stolons, which are in all Tunicata intimately connected with the growth and nutrition of the test.

The proximal surface of *Botryllus*, being fixed, presents so great antroversion of the mass as happens in *Pyrosoma*, which, in consequence of being quite free,

admits of the full play of this tendency, aided by a more rapid development of the zooids. Moreover, it would appear, that the margin of the external opening of the common cloaca is permanent, progressively advancing with the newly formed zooids, thrusting themselves forwards between it and the parents from which they sprung. Whereas, in *Botryllus* or *Botrylloides*, where there is but one effective row of zooids bordering the cloacæ at any particular time, the margins of the openings must be continually undergoing repair and decay.

On comparing an expansion of *Botryllus* with one of *Flustra*, or any other polyzoon, it is curious to observe that the zooids lie virtually face downwards in the latter, and face upwards in the former; and this is certainly one amongst many points of difference existing between the *Polyzoa* and the *Compound Tunicata*, while it favours the view that the *Polyzoa* hold the same relationship to the *Brachiopoda** that the compound hold to the simple *Tunicata*, or, to extend the question, that a *Gorgonia* bears to an *Actinia*.

A test which is common to a number of individuals, *i.e.*, an *Ascidiarium*, affords the first bond of union or community occurring in *Tunicata*. The next is obviously the establishment of a common cloacal system; and, lastly, as it would appear, the most important condition, truly suggesting the designation compound, is the intercommunication of the *Pallio-vascular systems* of the zooids, either as connected with the process of gemmation alone, or with the nutrition of the common test. Systems of intercommunicating stolons, having no connection whatever with gemmation, frequently present themselves in the compound genera; thus, in *Leptoclinum* and others, they are simple, or simply branched without reticulation; while, in *Botryllus*, they are compound, reticulate, and open a communication between the zooids.

The cloacal, like the branchial aperture, may open upon the external surface of the common test, or it may open into a definite common cloacal system either directly or by a tributary canal. The character and arrangement of the common cloacæ are of great importance in classification, and it is much to be regretted that so little definite information respecting them is to be found in systematic works. Whenever the opportunity presented itself, I have always endeavoured to unravel their curious schemes of arrangement, and often found it a matter of great difficulty; but so far as I have been able to generalize them, they are given in the following classification, which will be also found to afford a simple exposition of the leading features of the *Tunicata* as a whole.

As a matter of course, many characters in the minor distinctions here employed must, and have been, previously adopted by others. Thus, Mr W. S. M'Leay and Dr Fleming have passed in review nearly all the available characters in the Simple Tunicata, and M. Milne-Edwards has done the same with the

* The ventral valve being the valve of attachment in the *Brachiopoda* as in the *Polyzoa*.

social and compound groups. I hope, therefore, that this will be sufficient apology, without complicating the table by continual reference to authority.

CLASS TUNICATA.

Marine molluscoid animals, fixed or free, invested by an outer coat or tunic of variable consistency, and communicating with the exterior by an inhalent and an exhalent orifice for respiratory currents, conveying also the materials used as food; having a ciliated and variously modified pharynx adapted for respiration, and an atrial chamber for the discharge of the respired water and excretions, a reversible circulation, sexes combined, and the power also of developing by gemmation, which property pre-eminently distinguishes them from the mollusca proper.

SUB-CLASS I.

ANIMALS FIXED OR STATIONARY.

I. *Branchial membrane closely adherent, or more or less perfectly sac-like, simply areolated, or distinctly retiform; the meshes disposed in many transverse series without non-ciliated supporting bars.*

1. Gemmæ springing directly from the parent with a temporary bond of union, ... } SIMPLE TUNICATA.

A. Branchial membrane adherent, not forming a distinct sac. Tentacula, *o*. Branchial folds transverse, ... } Genera. *Pelonaia.*

B. Branchial membrane more or less perfectly sac-like.

1. Tentacula *o*, ... (?) *Chelyosoma.*

2. Tentacula simple, liver rudimentary, animal sessile (erect or recumbent).

a. Branchial folds, *o*, ... *Ascidium.*

b. Branchial folds longitudinal.

1. Branchial aperture higher than the cloacal, . *Cynthia.*

2. Apertures on the same plane.

a. Protected by a D-shaped apercular fold of the test common to both, ... *Peroides.*

b. Simple or naked.

1. Ovary and testis on right side, . . *Pandocia.*

2. Ovary and testis on the left side, . *Dendrodoa.*

3. Tentacula compound. Branchial folds numerous, longitudinal; liver well developed.

a. Sessile (erect), apertures on the same plane, . . *Cæsira* and *Molgula?*

b. Pedunculated (pendulous) cloacal opening, the higher or subterminal, ... *Boltonia* and *Cystingea.*

TABLE OF CLASSIFICATION—*continued.*

2. Gemmæ springing separately from a definite "ascidiarium (*Hux.*), and communicating indirectly through a central common vascular system, . SOCIAL TUNICATA.

A. Zooids branchial (*Hux.*), pedunculated, springing from a scandent or repent corneous axis, with a central permanent vascular system common to the offsets, ... Genera. *Perophora.*

B. Zooids intestinal (*Hux.*)

1. Pedunculated.

a. Standing upon a repent ramose cartilaginous case, with a central canal (usually not permanent), communicating with the offsets, ... *Clavellina.*

b. Developed centripetally on an erect, simple, cylindrical stem, permeated by numerous longitudinal canals communicating with the offsets, though rarely with each other, . *Chondrostachys.*

2. Sessile, clustered in irregular circles on a simple depressed axis, *Syntethys.*
And, probably, ... (?) *Diazona.*

3. Gemmæ arising separately from the parent, with or without vascular intercommunication, but always immersed in a common test or "ascidiarium," . . . . . COMPOUND TUNICATA.

A. Pallio-vascular system simple, *i.e.*, not intercommunicating. Genera.

1. Excretory aperture opening directly upon the surface. *Cloacæ* 0.

a. Biabdominal (*Polyclinian*). Zooids in irregular circles, one above another, ... *Sigillina.*

b. Abdominal (Didemnian).

1. Zooids in one or two rows at unequal distances from centres, ... *Distomus.*

2. Zooids without distinct circumscription.

a. Abdominal viscera beside the thorax, . *Eucælium.*

b. Abdomen pedunculate, . . . *Didemnium.*

2. Excretory aperture opening into a common cloacal system.

a. Cloaca superficial reticulate, with tributary canals diverging from the zooids in the intervening spaces.

a. Thorax double, ... *Diplosoma.*

b. Thorax simple, ... *Leptoclinum.*

b. Cloacæ deeply excavated, dendritic, formed by the confluence of converging canals.

1. *Biabdominal* (Polyclinian).

a. Systems diffuse.

α. Branchial aperture 6-rayed, . . *Amarœcium.*

β. Branchial aperture 8-rayed, . . *Parascidium.*

b. Systems circumscribed.

. Without central cavities, . . . *Aplydium.*

. With central cavities, . . . *Polyclinum.*

TABLE OF CLASSIFICATION—*continued*, ... COMPOUND TUNICATA.

	Genera.
B. Palliovascular system intercommunicating.	
1. Excretory apertures opening directly into punctate, linear, or reticulate cloacæ.	
a. *Biadominal* (Polyclinian).	
1. Systems single, isolated, or gregarious in whole relief, pendunculated (?), ...	*Synœcium.*
2. Systems numerous, masses crusting.	
a. In half relief projecting above the surface,	(?) *Sidnyum.*
b. Not projecting above the surface, . .	*Polychnoides.*
b. Thoracic (Botryllian).	
a. Cloacæ punctate, ...	*Botryllus.*
b. Cloacæ linear or reticulate, ...	*Botrylloides.*

SUB-CLASS II.

ANIMALS FREE LOCOMOTIVE. PELAGIC TUNICATA.

	Genera.
II. *Branchial membrane, sac-like, with transverse slits in single longitudinal series, strengthened by longitudinal non-ciliated rods, apertures terminal or sub-terminal,*	*Pyrosoma.*
III. *Respiring by an upper and a lower gill-band, connected with each other laterally and with the walls of the atrium; having branchial slits, but no supporting longitudinal rods. Apertures terminal,*	*Doliolum.*
IV. *Respiring by a central and inferior gill-band with free borders, and transverse ciliated stripes, but without slits or rods. Apertures terminal or sub-terminal.*	
1. Intestine short, and simply folded upon itself.	
A. Sexual zooids concatenated in chains.	
1. Ciliated stripes with pouch-like recesses at their outer ends,	*Pegea.*
2. Ciliated stripes of gill-band, simple, ...	*Salpa.*
B. Several zoids permanently united in circles, as in *Pyrosoma*, . .	*Pyrosomopsis.*
2. Intestine extended directly forwards from a cœcum-like proventriculus. The anus, and with it the ejaculatory duct, opening near the branchial orifice,	*Orthocæta.* (Salpa pinnata.)
V. *Pharynx ciliated below, without a distinct gill-band. Branchial slits reduced to two ciliated openings on the sides of the rectum. Apertures approximated hæmally,*	*Appendicularia.*

The lobing, and other particulars connected with the apertures, afford simple and easily recognisable characters, which are often distinctive of genera; but, as the Table might be rendered too complex by their introduction, I have omitted them. They may, however, be readily supplied by the student for his own convenience. I have drawn up the preceding scheme of classification chiefly with the view of illustrating the principles set forth in the first part of the paper. My own doubts I have expressed with a query (?), and even independent of these

many errors may be patent to zoologists, better conversant with the Tunicata than myself. Such, indeed, can only be expected to occur in thus dealing with so comprehensive a subject. I believe, nevertheless, that our acquisitions, as well as those things which are yet desiderata in the interesting study before us, are made much plainer in an attempt of this kind, however imperfect, than perhaps in any other way. The following genera may require brief comment in reference to their zoological characters, and the position assigned to them in the Table:—

Chelyosoma.

The absence of branchial tentacula in this genus, and what has been already said of it, p. 171, suggests a position for it near *Pelonaia.*

Peroides (mihi).

Peroides is an Australian genus discovered by me on the Bellona reefs, lat. 21. 51. S., long. 159. 28. E., but as I have given an account of it, with figures, to the Linnean Society, I need only allude to its leading features, which are these,—animal recumbent on the left side; apertures simple on the same plane, and protected by a D-shaped operculum, composed of an indurated fold of the test common to both.

Cynthia, Cæsira and *Molgula.*

The term genus was received by the earlier zoologists in a much wider sense than that to which we now confine it; and, as might be expected, this has since given rise to much confusion, requiring considerable research to clear up satisfactorily. Thus, under the head of *Cynthia*, several distinct genera were included by Savigny, and we consequently find that the restricted genus, as understood by Professor Forbes, is represented as having a "circle of tentacular filaments," *i.e.*, simple tentacula; whereas Dr Fleming gives *Cynthia* the character of "Tentacula compound," which could only be applied to a very different genus. It is also remarkable that without any reference to Professor Forbes's genus *Molgula*, in a paper read before the Linnean Society and published in the Transactions, I described two Australian Ascidians accurately conformable to Savigny's genus *Cæsira*, and which, singularly enough, very closely represent the two British species of *Molgula* described by Professor Forbes; and my impression has ever since been, that *Cæsira* and *Molgula* are synonyms of one and the same genus. Knowing, however, the great and respected authority which this view calls into question, I cannot be positive on the subject, but merely direct attention to it in the hope of setting it right.

Chondrostachys (mihi).

This name I have given to a very beautiful social Ascidian, which I first

dredged up in deep water in Bass Strait. The particulars of its anatomy, with several figures, were published in the Ann. and Mag. Nat. Hist. at the time; and it has since been obtained by Mr F. N. Rayner, R.N., late surgeon of H.M.S. "Herald," in the neighbourhood of Torres Strait. It may be remarked, that the whole mass is quite colourless and pellucid, but in other respects it is sufficiently described in the table.

Diazona.

It has often appeared to me that *Diazona* was more closely related to the Social than to the Compound Tunicata, from the similarity of its structure to that of *Syntethys*. The position assigned to it in the table is quite in accordance with the definitions there given.

Diplosoma (mihi).

This genus I have found both in the S.W. Pacific and in the West Indies. The mass is gelatinous and filmy, and the thorax in the zooids is double—a condition that may be traced even long before the young escapes from the ovum. It was described and figured in the Transactions of the Linnean Society as the first step towards the still more paradoxical development of the ovum in *Botryllus* and *Pyrosoma*.

Synœcium and *Sidnyum*.

Though certain general considerations have induced me to place these genera near the true *Botryllians*, I cannot yet say whether they have an inter-communicating palliovascular system or not; and as it would be equally wrong, should such be the case, to place them anywhere else, they may remain where they are until the question is settled, and the (?) is removed.

Polyclinoides (mihi).

I have applied this term to an Australian genus of compound Ascidians, distinguished from *Botryllus* by having a distinct abdomen, and the generative organs pedunculated, forming a post abdomen,—and from *Polyclinum*, by possessing a compound palliovascular system, and fewer zooids surrounding the cloacæ; besides which the mass is thin and encrusting, as in *Botryllus*. The branchial aperture is considerably elevated, and six-lobed. The superior margin of the cloacal opening is much produced, and three-lobed at the extremity, which is simple in *Botryllus*.

Pyrosomopsis (mihi).

A name which I have applied provisionally to a curious genus of Salpians, in

which the gemmæ are permanently united in circles, and thus described in my notes:—"This form presents seven perfect Salpians disposed in a circle, radiating, with their posterior extremities approximated near the centre, and enveloped in a common membrane, like a medusiform disc.

"Respiratory openings occupying one surface of the disc, the anterior or orifice of ingress being marginal, and the posterior central. The testis and a diverticulum of the alimentary canal forming two elongated papilliform processes, projecting from the posterior extremity of each animal (but within the common envelope), and convening towards the centre of the disc. Intestine forming an open arch, with its convexity directed backwards, across which the long and narrow duct of the testis passes to open into the respiratory chamber. Each zooid bearing a solitary embryo communicating with the sinus system, and enclosed in a spherical capsule.

"Otolithic sac rather prominent, and projecting from the under surface of the ganglion.

"'Hypopharyngeal band,' or gill and muscular system in every respect as in ordinary *Salpæ*.

"The arrangement of the zooids in this case very much resembles that in *Pyrosoma*, more especially the primary circle of the latter; but the whole economy in other respects is *Salpian*. Taken in the towing-net, lat. 32. 53 S., long. 156·00 E., and subsequently in other localities."

Orthocœla (Salpa pinnata of Authors).

It would be quite as philosophical to include the whole of the cheilostomatous Polyzoa under one generic term, as to group all the strikingly diversified animals of the Salpian type as mere species of the genus *Salpa*. Under a similar impression, Savigny set about the establishment of seven new genera; but their claims as such do not seem to be generally admitted by zoologists.

With regard to the genus here under consideration, I find the following observations amongst my notes:—

"While cruising in the S.W. Pacific, we frequently met with a Salpian answering perfectly to the description of *Salpa pinnata*, though this is rather indefinitely given by M. De Blainville; and in our late voyage to the West Indies in H.M.S. 'Icarus,' apparently the same species frequently made its appearance in the towing-net, together with *Salpa zonaria*, and numerous other species. It appears to me that the said *S. pinnata*, if I have correctly identified it, deserves to be removed from the genus *Salpa*, and placed by itself, until other species are added to it with the same generic characters. The propriety of this step, however, may be better seen when the principal features of the anatomy of the animal have been passed in review.

"In some instances specimens attain a length of about two inches, with a

proportionate bulk, but in general they are much smaller; and from the delicacy of the test, the body very readily collapses, offering a serious obstruction to the study of their internal anatomy.

"The test may be described as semi-gelatinous, smooth, and, with the exception of a long cylindrical process, springing from the dorsal region anteriorly, it is everywhere free from angles and projections. The body is full, and rounded in the middle, from whence it rather suddenly tapers towards the anterior and posterior openings, which are simple and very small, closed by numerous narrow and encircling muscular bands.

"The muscular system is very complex, consisting of circular, diverging, and inter-communicating bands; four, in particular, ascend at nearly equal intervals upon the dorsal process. The anterior pair, however, soon unite, and form a single slip, which is connected with the others by transverse fibres.

"As well as I could make out, the lining membrane of the respiratory chamber passes across the base of the dorsal process without entering it.

"The gill-band is simple and lengthy, communicating at the oral end with a rudimentary epipharyngeal extension of the same structure. The ciliated stripes are very narrow, and do not increase much in width outwardly. The curved ciliated sac at the anterior end of the gill, and the fine ciliated line connecting this on either side with the anterior extremity of the endostyle, were very distinctly visible in the specimens examined.

"The nervous ganglion occupies a position corresponding with the anterior fixed end of the gill, but on the inferior aspect of the body. The auditory sac, otolithes, and pigment, present nothing unusual.

"The endostyle is long and narrow, extending from a point above and to the right of the heart to near the branchial opening. The mouth is situate at the posterior extremity of the gill-band, and leads into a distinct œsophagus, which in its turn opens into an elongated stomach, with a large cæcal pouch, directed upwards and backwards, intervening. The stomach takes a straight course directly forwards, and gradually passes into the intestine, which is slightly curved downwards anteriorly, where it ends in the vent, immediately below the base of the dorsal process. A small transparent and highly refracting duct, arising by several radicles in the vicinity of the rectum, courses backwards on the right side of the alimentary canal, and having arched to the left over the cæcum or proventriculus, it opens into the commencement of the stomach. The office of this system, first made known by Professor HUXLEY, is not even yet well understood, though I have myself traced it through all the divisions of Tunicata, even in the microscopic compound forms.

"The heart, compared with the size of the animal, is small, and situate near the posterior extremity of the '*endostyle*,' on the right side of the stomach.

"On either side of the ventral surface of the body there is a narrow cylin-

drical organ—the 'violet-line' of M. De Blainville—about half the length of the endostyle. These may possibly be renal, but I am not aware that any definite office has been assigned to them, though corresponding structures certainly occur in numerous other cases also. They consist of a membraneous envelope, containing an abundance of large cellular bodies, with transparent spherical granules of uniform size, both within and amongst the cells.

"The testis consists of a number of long tubular cæca, forming an elongated whitish mass, lying immediately beneath the brownish-yellow stomach and intestine, and terminating anteriorly in a fine ejaculatory duct, which opens into the branchial chamber a little behind the vent. The gemmiparous zooids are developed singly."

Explanation of Plate IX. figs. 1, 2.

Fig. 1. Vertical section of a single system of *Botryllus*, to show its relationship to *Pyrosoma*.

a'. Floor of the cloacal chamber.

a. Surface of attachment.

b'. Lateral extension of the ascidiarium.

b. Border of the cloacal opening, in the formation of which the zooids themselves take part.

Fig. 2. Theoretical diagram, illustrating the morphological difference between *Chondrostachys* A, as a social, and *Pyrosoma* B, a compound *Pelagic Tunicary*.

a. Summit of the axis of *Chondrostachys*, corresponding with the apex of the free *Pyrosoma*.

a'. Cloacal side of the apex of *Pyrosoma*, equivalent to the point of fixity of *Chondrostachys*.

b. Growing circumference, or border of the cloacal opening of *Pyrosoma*, considered as an antroversion of the axis or ascidiarium.

The arrows indicating the orifices of ingress and egress show that there is no necessary inversion or alteration of the relations of the zooids, either as to each other or to the ascidiarium, in the theory of antroversion, as applied to *Pyrosoma*, or of retroversion as regards *Chondrostachys*.

Fig. 3.

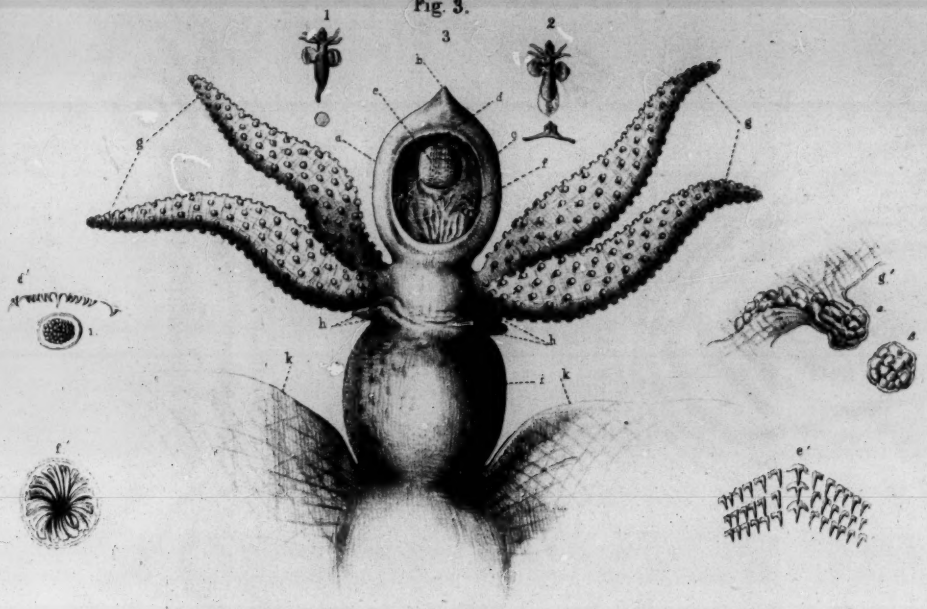


Fig. 4.

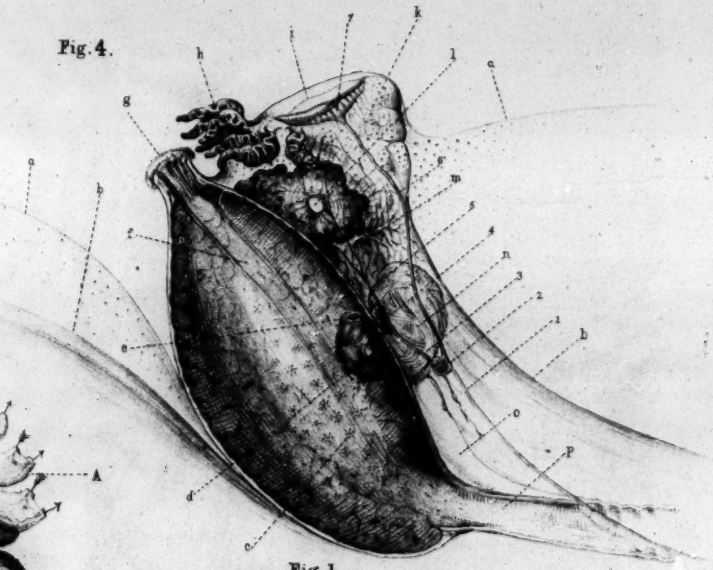


Fig. 2.

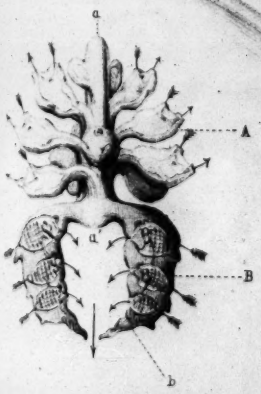
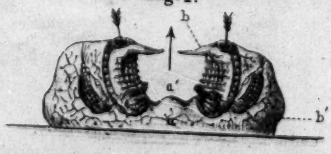


Fig. 1.



REINAGLE R.H. DELT.

F Schenck 12 Rl Exchange Edinr

XVII.—*On the Zoological Characters of the Living* Clio caudata, *as compared with those of* Clio borealis *given in Systematic Works.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (Plate IX. fig. 3.)

(Read 5th January 1863.)

Great credit is due to those observers who have been enabled to give to science both clear and comprehensive views of the anatomy of creatures which have only been presented to them in a spirit-preserved, opaque, brittle, and contracted state; for it is certain that their penetration in this respect could not be successfully brought into exercise without the aid of much knowledge, both bibliographical and practical. Yet there are many whole animals, and, in particular, parts of animals, which must be seen in the living state, to be at all comprehended by even the most brilliant mind; and this fact has induced me to make drawings and notes of many interesting matters connected with the *pelagic mollusca*, when the living animals fell casually under my own observation. In the present communication, however, I shall confine myself to the genus *Clio*.

Good figures are in general more valuable than even lengthy descriptions; for though it would not be very easy to make a good figure from an imperfect description, a very excellent description may be formed from an indifferent figure. On consulting all the figures of *Clio* available to me, I found most of them far short of nature, and all quite incapable of affording a just conception of the living and fully expanded animal; nor, indeed, can I say that any descriptions, of the members of the genus extant answer much more than the purpose of mere recognition, in a popular sense.

In the widest sense of the word, the physiognomy of *Clio caudata*, in the expanded state, is as remarkable as that of any animal in creation. The head, or that enlargement in front which is separated from the body by a slight cervical constriction, is fronted on either side by two small tentacula, one of which has been supposed to be an eye pedicle, though there is no proof that *Clio* enjoys one whit more visual faculty than any other Pteropod. CUVIER remarks, that "some have asserted the existence of eyes;" and subsequent writers say that those in *Clio*, though minute, have a very complete organization. For my own part, however, I cannot say that I have ever been able to detect them, though I should be sorry, on this ground alone, to affirm that they are not present. But the little tentacula just noticed are quite insignificant, in comparison

with the *four* long, conical, and perfectly retractile arms, which spring immediately in front of them. These arms are dotted over with minute rudimentary suckers, arranged quincuncially, and from between them arises a curious shovel-shaped and retractile proboscis. This latter organ is broad and depressed, terminating in a point anteriorly, and having, on the upper surface, a large oral opening with a thin circular lip. The dental armature of this proboscis is, with the exception of that of *Pneumodermon*, the most formidable amongst mollusca. In the middle of the floor of the mouth, and quite exposed from above, is a globular tongue, mainly composed of two broadly oval cartilages, overlaid with a lingual pavement of teeth, leading into a short saccule posteriorly. The median series of plates are crescentic, with the concavity directed backwards, and armed with a principal conical fang in the middle, and a rudimentary one on either side. The lateral plates are numerous, and bear a simple conical tooth on the inner side, with a small shoulder externally.

In front of the tongue, the anterior or inferior lip is furnished with a transverse row of minute hooks, one in the centre, and an outlying one on either side, being larger than the rest. Behind the position of the tongue, and on each side of the oral cavity, is a shallow evertile pouch, lined with large gently curved conical teeth, which appear to enjoy a twisting or cork-screw action while they are being everted, so as to pierce and secure living prey when both cheek-pouches are brought forcibly in apposition. The upper or posterior lip is wholly unarmed, so as to admit of its expansion in receiving prey thus seized, probably torn up and forced backwards into the gullet by this most efficient and wonderful armature. Speaking of the lateral oval plates of *Pleurobranchæa*, SIEBOLD observes,—"To the same category belong the spines which ESCHRICHT found upon the pharynx of a *Clio*, and described as jaws placed laterally, as in the *Articulata*, and furnished with long sharp comb-like projections or teeth," giving a very false idea of the organs just noticed in *Clio caudata*, if they are indeed the same or similar in the northern *Clio*.

The description of the head, tentacula, arms, proboscis, and dentition of *Clio caudata* just given, for the simple reason that it has been taken from life under favourable circumstances, will be found to agree but little even with the recognised characters of the *genus*, at least as they occur in systematic works. I have observed, moreover, that without any other apparent distinguishing feature some of the southern *Clios* had simple tapering tails (*Clio à longue queue* of the French), while others have three prominent ridges (the dorsal one frilled) meeting in a point posteriorly, so as to give a depressed trigonal section.

On comparing a *Clio* with a thecasomatous pteropod, *Hyalæa* for example, the mouth and dental system in the former case will be seen to occupy the most advanced position, while in the latter they have receded so far within the limits of the foot as to give the animal an acephalous appearance, and this view

alone warrants the whole of the *Pteropoda* to be included under the term *Cephalophora*.

The ease with which naturalists interchange names without any conciliatory explanation, is certainly a great stumbling-block to the beginner. Thus, if he were to fall in with *Clio caudata*, as I have done, and immediately consult all the authorities within his reach, in some modern work he may find a figure of the creature he is in quest of, though denominated *Clio australis*, but on still further inquiry as to the authorship of that figure, he discovers that it is a lineal descendant of a figure of *Clio à longue queue*, given in the "Voyage de la Bonite." Now, on comparing this with De Blainville's figure of *Clio australis*, he may probably perceive even generic differences between them; but, continuing his search, he looks over one or two other modern books, and one can scarcely say whether his mind is settled, or his confusion is made still greater, to encounter stereotype repetitions of the figure of the said *Clio australis* of De Blainville boldly named *Clio borealis*.

If there is indeed such an animal as *Clio australis*, and M. De Blainville's figure is a correct representation of it, it is obvious enough that the species *borealis* and *australis* are members of the same genus; but as both differ so remarkably from the so-called *C. caudata*, and the broad trigonal-tailed species, of which I have given a short notice and figure, it strikes me that there are ample grounds for the establishment of a new genus, to receive, at least, the two last-mentioned species, while at the same time it will become apparent how little the nature of the respiratory system can be depended upon for generic characters, though it may be of great specific value.

Not desiring to add unnecessary names to a list already large, I merely submit the views above expressed to the consideration of zoologists, who may form their own judgment on the matter. The more important characters are simply as follows:—

Tentacula, two, minute, on either side of the head anteriorly; *Cephalic arms*, four, perfectly retractile, long, and conical, with sucker points still more rudimentary than those in *Clio*; *Proboscis* exsertile, broad, depressed, and pointed in front; *Oral aperture*, superior, oval, with minute lower lip uncini; *Cheek pouches* shallow, but evertile, and furnished with curved conical teeth; *Lingual pavement* broad, with a median series.

Setting the above characters aside, however, the original object of the paper has been to show the great importance of the study of all soft, collapsible, and contractile animals in the living and expanded state; for I am much of opinion, that the Northern *Clio*, examined in this way, will be found to present very similar characters, or such as could not be arrived at by the most patient dissection of spirit-preserved specimens.

Explanation of Plate IX. fig. **3.**

1. *Clio caudata*, &c.
2. The second species alluded to in the text. Both are about the natural size; and the appearance presented by a transverse section, near the caudal extremity, is given immediately below each figure.
3. Enlarged figure of the head, proboscis, and cephalic arms of *Clio caudata* in the expanded state.

a. Proboscis, &c.
b. Its pointed extremity.
c. Oral cavity.
d. Anterior or lower lip, armed with teeth.
d'. Do., magnified.
e. Tongue with lingual pavement.
e'. Portion of the lingual dentition magnified.
f. Lateral or cheek pouches, with their curved conical teeth.
f'. Do., magnified.
gg. The four cephalic arms, with their minute suckers.
g'. One of the suckers magnified: 1. lateral view; 2. in face.
h. The four minute tentacula, of which the posterior pair is regarded as eye pedicles.
i. Head.
k. Swimming fins.
l. Auditory sac, with otoconia.

XVIII.—*Notes on the Anatomy of the Genus Firola.* By JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MACLAGAN. (Plate IX. fig. 4.)

(Read 5th January 1863.)

In a successful haul of the towing net, off the Island of Sardinia, I obtained a very beautiful *Firola*, which gave me a good opportunity of testing the truth of my former conclusions with regard to the economy of the Heteropoda in general, and of *Firola* in particular; and I have great pleasure in submitting the facts arrived at, with the accompanying enlarged figure of the visceral nucleus and the neighbouring parts, to the Royal Society of Edinburgh, as an appendix to my paper "On the Anatomy and Classification of the Heteropoda," already brought under its consideration.*

I find that the relationship between *Firola* and *Firoloides* is even closer than I had originally imagined, and with the exception of the presence of gills in the former genus, nearly every anatomical point occurring in one may be distinctly traced out in the other, only differing in relative characters.

To avoid unnecessary repetition, I shall simply explain the accompanying figure, and enlarge in passing upon any matters of importance that may suggest themselves.

Explanation of Plate IX. fig. 4.

a. Integument.

b. Muscular sheath.

c. Visceral nucleus, including the stomach, intestines, liver, testis, &c.

d. Vesicula seminalis. All these organs are invested by an oval capsule, with its long diameter directed obliquely downwards and forwards, having the stomach at the lower end, and the trumpet-like vent above, and in front, lying in relation to the Sinus-system, the heart, great vessel, and œsophagus.

e. Seminal opening.

f. Intestine.

g. Anal orifice, plicated internally, and richly ciliated.

h. Gills, with their zig-zag folds, giving the organs a pinnate appearance, which is often incorrectly given in figures of the Heteropoda.

i. Ciliated fossa fronting the gills, the cilia disposed in a circular band, and undulating in the direction of the arrows.

k. Sinus-system, communicating directly with the auricle of the heart (*m*), and everywhere intersected with muscular bundles and fibres.

l. Circular opening, leading into what I have been induced to regard as an inverted mantle-cavity, whose walls are much lobulated and highly contractile, so that the sea-water is ejected through the opening which is guarded by a sphincter. A *vis e fronte* effect

* Trans. Roy. Soc. Ed., vol. xxiii. p. 1.

would appear to be exerted on the blood entering the sinus-system; for during the contraction of the muscular fibres of the latter, and the systole of the auricle, diastole of the mantle-chamber is taking place. A great deal has been said about vital expansion in human physiology, but the instance just given is one in which the expansion of one chamber is facilitated by the contraction of another, having no internal communication with it; and this I believe to be the office of the remarkable organ in question, as an appendix to the auricle, which communicates so freely with the sinus-system.

m. Auricle of the heart, or rather its proper position; for its limits do not appear to be accurately defined.

n. Ventricle, with its closely interlaced muscular substance.

o. Principal vessel.

p. Œsophagus.

q. Stomach.

1. Long nerve-trunk, extending from the pedal ganglion to No. 2.
2. A small nodule of neurine, which, as I imagine, is chiefly respiratory, though it also sends filaments to the great vessel and œsophagus.
3. A stout commissural-nerve, connecting No. 2 with
4. The cardiac and visceral ganglion.
5. A conspicuous nerve, supplying the sphincter and radiating muscular fibres of the mantle opening, arising, as in *Firoloides*, from the ganglion No. 4.
6. A large commissural-nerve, passing from No. 2 to
7. A large saucer-shaped ganglion, lying immediately below the ciliated fossa (*i*). I can now safely say, that a body corresponding to this, though much smaller in *Firoloides*, is truly nervous; but I cannot hazard any theory as to its office, though it is obviously in some way connected with that of the ciliated fossa,—probably with respiration.

In my former paper, I stated my reasons for believing that the sexes are separate in the *Heteropoda*, and I am sure that this point will require no further proof, though, as far as the sanction of authority is concerned, the more general conviction even now is, that the sexes are combined. The uncertainty existing on the subject amongst zoologists, cannot be better represented than by the following quotation, verbatim, of Note 9, p. 264, Dr Burnett's translation of Siebold's "Anatomy of the Invertebrata":—

"The penis is double, and at the right side of the base of the visceral sac, with *Carinaria* and *Pterotrachea* (Milne-Edwards, Ann. d. Sc. Nat., xiii. 1840 p. 195; xviii. p. 323, Pl. x., fig. 3). Quoy and Gaimard (Voy. de l'Astrolabe, Mollusq., Pl. xxviii. fig. 10; or Isis, 1834, Taf. iii. fig. 10), have figured a long bifid penis, with *Phyllirrhoë amboinensis;* and so, if with the other Heteropoda, the penis is not retractile, as appears to be the case with *Carinaria*, according to Milne-Edwards this species would be a male; while *Phyllirrhoë bucephalus*, figured by Peron (Ann. du Museum, xv., fig. 1; or Kosse, De Pteropodum ordine, Diss. fig. 1), apparently without a penis, would be a female, although D'Orbigny (Voy. dans l'Amer. Mér.; or Isis, 1839, p. 519), regards this genus as hermaphrodite. With *Atlanta*, there is a simple, pointed penis on the right side

of the neck, directly near the arms; but as RANG (Mém., *loc. cit.*, p. 378, Pl. ix.; or Isis, 1832, Taf. vii.) has found this penis with all the individuals he has examined, it may be questioned if the sexes are really separate with this Heteropod.

"The internal genital organs of *Atlanta* and *Phyllirrhoë*, should be thoroughly studied for the elucidation of this point."

There are a good many references made in this note, but no satisfactory information is derived from any of them. The paucity of female *Atlantæ*, as indicated by M. RANG having only encountered the males, is perhaps the most interesting point. This was for a long time my own experience also, but on several occasions I have been fortunate enough to obtain indisputable females.

Though the position of *Phyllirrhoë* amongst the Nudibranchs and not with the Heteropods, has been long recognised by zoologists, I may make the following remarks in passing, to clear up a desideratum in the note. First of all, the bifid penis of this mollusc is perfectly retractile, and its testicular follicles are included within the saccule of the ovaria, so as to form perfect hermaphrodite glands. The primary vas deferens and oviduct are blended together; and subsequently to the formation of stout fusiform spermatheca, the common tube divides into outer oviduct and outer vas deferens, the one passing into a wide glandular uterus, and the other into one of the limbs of the intromittent organ. The bisexual nature of the animal was known to CUVIER, though he could not have given this interpretation of it.

Having examined some hundreds of recent Heteropoda, particularly the species of *Atlanta*, it struck me as being very odd, that I had never in a single instance succeeded in tracing a vas deferens onwards to the external male organ. This finally led me to believe that the penis was imperforate, and as in *Onchidium*, *Aplysia*, *Melo*, and numerous other Gasteropoda, far in advance of the spermatic opening, both being held in communication by a ciliated groove, more or less capable of being converted into a canal. Of the truth of this doctrine, I am now fully convinced, for the outlet of the spermatic duct, as given in the figure, and which is even quite prominent in *Firoloides*, is so palpable as to admit of no question; and on closely examining the duplex penis, I find by the simple test of focussing, that the so-called perforation and internal canal are nothing more than an external, deeply-grooved, and ciliated tract, the office of which is obvious enough.

XIX.—*On the Structure and Optical Phenomena of Ancient Decomposed Glass.*

By Sir David Brewster, K.H., D.C.L., F.R.S., &c. (Plates X., XI.)

(Read 5th January 1863.)

The disintegration of solid bodies by means of active or feeble solvents, or by those invisible processes which go on during long periods of time, has, so far as I know, been studied neither by the chemist nor the natural philosopher. In 1837 I submitted to this Society a paper "On the Optical Figures produced by the Disintegrated Surfaces of Crystals,"* containing experiments which I believe have not been repeated, and results which no person has attempted to explain.† Since that paper was published, my attention was called to the structure and properties of decomposed glass, in consequence of having had occasion to study the action of its coloured films, in absorbing definite parts of the spectrum. The specimens, however, which I received for this purpose from the late Marquis of Northampton, Mrs Buckland, and Mr Children, and referred to in my "Experiments on the Connection between the Phenomena of the Absorption of Light and the Colours of Thin Plates,"‡ did not enable me to investigate the condition of the elementary films, and the process by which the various states of the glass were produced; but having received, while in Rome in 1857, very fine specimens from the museum of the Marquis of Campana, and more recently other specimens from Nineveh, from Mr Layard, I have been able to obtain the results contained in the following paper, and represented in the drawings which accompany it.

The decomposition of glass in its early stages is finely seen in the brilliant colours which cover its surface. These colours are those of thin plates, and tints of the third and fourth orders in Newton's scale are produced in the course of thirty or forty years on the inner surfaces of panes of glass in stable windows. The elementary films which display these colours are not in optical contact with the glass beneath them, though they adhere to it very firmly. They may be removed by the point of a knife; and in this state the powder is white by reflected light, and semitransparent when surrounded with water.

* Edin. Trans., vol. xiv. p. 164.

† Since this paper was written, I have received from Professor Von Kobell, of the University of Munich, a very interesting paper, entitled "*Ueber Asterismus und die Brewsterschen Lichtfiguren*," in which he gives an account of my experiments, and adds many new and important ones of his own, illustrated with three plates. It is published in the *Sitzungsberichte der Koniglichen Baierischen Akademie der Wissenschaften:* Sitzung der Math.-Phys. classe, 8 Feb. 1862. A very full and excellent abstract of this Paper appeared in the *Parthenon* of 6th Sept. 1862.

‡ Phil. Trans., 1837, p. 245.

Even when the glass has been exposed only to the air of the atmosphere, these coloured films are produced sometimes in twenty or thirty years on the surface of glass, whether formed by fusion or by artificial grinding and polishing. But as other surfaces of glass, similarly exposed, and of a much greater age, show no marks of decomposition, it seems evident that the rate of decomposition must depend upon the composition of the glass, and probably on the temperature at which its elements have been combined.

A very remarkable case of rapid decomposition presented itself to me in a fine plate-glass prism, made for Mr TALBOT by FRAUNHOFER at Munich, and presented to me by that distinguished philosopher. On the 23d March 1833 I made accurate drawings of its three faces, on each of which there was a circular spot, in which the decomposition showed itself in three rings or orders of the colours of thin plates. In one of these spots, half of which is cut off by the edge of the prism, the colours in 1833 were three orders, the innermost being a *yellowish-green* of the *third* order. This spot is about a quarter of an inch in diameter. Another spot about the same size is perfectly circular, the outermost ring being the *white* of the *first* order, rising in the centre to *purplish-blue* of the *second* order. The third spot, about the 16th of an inch in diameter, is perfectly circular, the innermost tint being a *red* of the *second* order. These colours, which are very difficult to be seen, are those observed at the polarising angle of the glass, when the reflected light is nearly extinguished by an analysing prism.

Upon examining these decompositions after an interval of thirty years, I cannot observe any change in the rings and colours. They seem to be fainter than before, and, what is very remarkable, two long irregular streaks of decomposition, one an inch long, and the other nearly two inches, have entirely disappeared. In the shortest of these lines of decomposition, the tints of some parts rose from *blue* to *yellow* in the longest, and in a very small circular spot the tint was only *blue*.

The pause in the process of decomposition in the three circular spots, and the disappearance of the long streaks, is very remarkable,—and the more so, as they must all have been formed during a period certainly not greater, but probably very much less, than twenty years. Is it not possible that the particles which have separated from the glass by the action of the decomposing cause, and ceased to be in optical contact with it, may have returned into optical contact when the decomposing cause no longer existed, as might have been the case with the prism if kept, as it has been, in dry air since 1833? That this is not an unreasonable supposition, may be inferred from an experiment published in 1816, in which one of the surfaces of a fissure made artificially in a thick plate of glass was so much separated from optical contact with the other surface, as to reflect totally light incident upon it at a certain angle. "After standing an hour the fissure began to disappear, and in the course of a day it was as

completely closed as if it had never been made,"* the surfaces having returned into optical contact.

When we examine the surface of decomposed glass, either before or after the removal of the film, we find it composed of an infinite number of cavities, in the middle of each of which the decomposition has begun. At this point a particle of glass, of a certain size, has detached itself, and a film is formed by the aggregation of other particles of the same size. This decomposition extends itself in all directions, but more quickly downwards beneath the first particle that was detached. The consequence of this is the formation of a cavity or cup, which would be part of a hollow sphere if the centres of decomposition were at great distances and few in number. In general, however, these centres are innumerable, covering the whole surface of the glass. From this cause, the decomposition round any one centre meets the decomposition round other centres, and the form of the cavity is an irregular polygon, the shape of which depends upon the distances of the centres of decomposition. The lines in which the decompositions meet one another resemble the meshes of a net, or the lines which separate the small films or partitions that confine between them small quantities of gas, forming the froth of champagne, beer, and other liquids.

When the cavities of decomposition are very numerous and equally diffused, they are often so minute as to be invisible, and their existence is shown by different degrees of roughness, as if the surfaces of the film had been ground, and sometimes by producing halos or rings round the flame of a candle.

In several specimens the surfaces of the films are perfectly specular, showing that particles of the same size have been uniformly detached from the original surface, and that the decomposition has not taken place around and beneath different centres.

When we consider the different states of fusion and cooling to which glass is exposed, whether it is formed by flashing, or blowing, or moulding, it is evident that the processes of decomposition must take place in spots or in lines where the elements of the glass have been less perfectly combined, or where the cohesive forces are more feeble.

In many films, where the decomposition has been pretty uniform, there are often numerous centres distant from each other, where very deep cavities have been formed. These cavities are perfectly circular, as in Plate X. fig. 1, and Plate XI. figs. 6, 7, 8, &c., and have different diameters and different degrees of depth. In other films these circular cavities are crowded together so closely as to occupy the whole of its surface, as in Plate XI. figs. 11 and 13.

In many specimens the centres of decomposition lie in straight lines, and near each other, as shown in Plate X. fig. 5. One cavity thus encroaches upon

* Phil. Trans., 1816, p. 73.

another, and a long groove is excavated, the bottom of which is formed of spherical cavities of different depths and breadths, depending on the distances of the centres of decomposition, and the rapidity with which the decompositions have taken place. The form of the grooves is thus very irregular, being wide at one place and narrow at another, and occasionally not rectilineal. When the centres of decomposition are very near each other and equidistant, the grooves are very narrow and shallow, and form straight lines of uniform breadth.

These grooves or lines of cavities lie in different directions, frequently crossing one another at various angles. In some specimens they are perfectly parallel, and are often not more than the 500th or 1000th of an inch in breadth, having the appearance of lines of uniform thickness.

When the cavities are of considerable size, and nearly or perfectly spherical, they sometimes present remarkable phenomena. Round one or more points of the spherical films or cavities, a fresh decomposition has taken place, and formed smaller spherical cavities, sometimes almost touching one another, as in Plate X. fig. 4, and Plate XI. fig. 6; and occasionally so numerous, that I have found from fifteen to twenty formed upon the same cavity. In some cases the decomposition has produced shallow cavities, which, from interfering with one another, have become polygonal. In some specimens this secondary decomposition has taken place at such a great number of points, that the infinitely small cavities which it has formed appear like specks of black powder, making the cavity more or less opaque, according to the distance of the specks, or the number of films of which the cavity is composed.

When the cavities do not interfere with one another, they are occasionally ellipsoidal, and egg-shaped, though formed from one centre of decomposition.

Specimens of these structures are given in Plate X. fig. 3, and Plate XI. figs. 7 and 12, seen by ordinary transmitted light. In Plate X. fig. 5, are shown circular cavities, and cavities in lines of uniform and variable breadth. The compound film consists of many elementary films, but only three or four are shown in the figures, as they alone are visible in the specimen. If we remove with a lancet those films in succession, we shall find that each film contains all the cavities existing in the compound one,—the spherical or polygonal cavity, whether deep or shallow, being joined to, or forming part of the film. When examined on one side all the cavities are convex, as shown in Plate XI. fig. 11; and when examined on the other they are concave, as shown in Plate XI. fig. 13.

In comparing this structure of the films with the process of decomposition shown in Plate X. fig. 1, it is not easy to understand how the concave or convex parts of the film are united to and form part of the general film. The process of decomposition having begun at one point, where the cohesion of the particles is the feeblest, it advances, forming round that point a number of spherical films, before the decomposition has commenced on the surrounding glass. When the surface

of the glass begins, or is ready to decompose, the spherical film then forming will extend itself over the glass, and form part of the general film. If the decomposition has begun at different times round different centres, and proceeded with different velocities, the spherical film in every cavity, whether deep or shallow, will extend itself over the general surface. If this is a correct account of the process, then we ought to find in every cavity a central portion having no connection with the general film, because formed previous to the decomposition of the general surface. When we remove the outer crust from the decomposed glass, we find this to be the case. The central portions are removed with the outer crust; and in some cases I have found the cavities filled with that portion, in consequence of no decomposition having taken place on the general surface.

It is difficult to comprehend the nature of the molecular forces under which these singular decompositions assume peculiar forms. There is no difficulty in understanding how, in a homogeneous surface of glass, decomposition round a centre should excavate a perfectly spherical cavity, and how its ultimate form should be changed into that of irregular polygons, by the interference of decompositions advancing from surrounding centres; but it is a singular circumstance that the glass should be in any one place in such a state of molecular incohesion, that the decomposition should follow a law which produces a perfectly ellipsoidal cavity. Nor does it seem less remarkable that after a cavity, either spherical or ellipsoidal, or irregularly polygonal, has been formed, spherical cavities should be formed on different parts of the films which compose them.

The preceding observations have been made on films of glass from Nineveh and Rome, after the opaque or outer films had been removed. Each film, therefore, has on one side all its cavities concave, and on the other side all of them convex, like a bundle of watch-glasses. Each film consists of many films, which can be separated by the thin blade of a knife or lancet, adjacent films differing from one another only in their thickness and colour. The films, though adhering with some force, are not in optical contact, as they freely admit water, and other fluids between them, and display the fringes of thin plates produced by the different thicknesses of the interposed films of air.

The mode in which the decomposition proceeds round different centres is shown in Plate X. fig. 1, taken from a specimen of glass from Nineveh. The same structure is less perfectly seen in fig. 2, taken from glass found also at Nineveh. When the glass has lain on damp earth, the decomposition has taken place very irregularly round separate centres. It consists of two thick crusts of opaque vitreous matter, enclosing a plate more or less thick of pure glass, with deep excavated cavities on both of its surfaces. A specimen of this kind breaks by the pressure of the finger like the slice of an apple. In a finely-shaped glass bottle found near Cheltenham, and presented to me by Mr SYDNEY DOBELL, a thick grey crust of iridescent films, of uniform thickness, covers the whole of its surface, and

gives it the appearance of granite. The iridescence of the films is, however, distinctly seen; and when the crust is removed the transparent green glass appears, with its excavated surface.

The decomposition of glass goes on rapidly in water. In a wine bottle brought up from the wreck of the "Royal George," very considerable films were formed on its surface. Bottles of the form called "magnums," which were found in the Cherwell, near Oxford, by Mr R. THOMAS, were encrusted with films of considerable thickness. From one of these Mr THOMAS detached a circularly-shaped film about half-an-inch in diameter, which had a spiral crack proceeding from its centre, and having eight or nine circumvolutions. This film, which was one of unusual thickness, was a single one, as is shown by its producing none of the colours of thin plates at any inclination, and by its action on common and polarised light. Though perfectly transparent, its surface is not specular, but mottled with the colours of striated surfaces, the points of decomposition by which they were produced being visible by high magnifying powers.

From the facts I have mentioned, it is evident that the decomposition of glass is accelerated when it is exposed to the action of acids and alkalies in damp localities, and particularly to the ammonia so abundant in stables.

M. BRAME, of Paris, having seen a notice of the decomposed glass from Nineveh, which I had read at the British Association some years before, was led to submit thick plates of glass to the action of powerful solvents. In a short time circles were produced analogous to those which I had observed in the Nineveh specimens. "I have produced," he says, "upon glass, circles regular and irregular, insulated or concentric, notched in the interior (crystals incomplete or altered). To do this we must immerse fragments of thick glass into a mixture of fluoride of calcium and concentrated sulphuric acid, or expose them to the action of the vapour of fluorhydric acid. In the centre of the circles we find almost always either a small cavity, or a small nucleus. At the same time one or more fractures of the glass are covered with striæ and small anfractuosities, &c.

"The observations of M. BREWSTER and mine seem fitted to throw much light on the formation of siliceous spheres (orbicules) in sandstone and agates, which are the subject of a fine investigation by ALEX^RE. BRONGNIART; and on the other hand, this inquiry seems to connect itself directly, like that of the circles of glass themselves, with the encyclical formations which I have discovered."*

The colours exhibited by films of decomposed glass, when examined in ordinary light, are of the most brilliant description. There is no colour in the world of flowers which is not found, in all its beauty, in the light transmitted by these films; while in the light reflected from them the tints are equally varied, and of a metallic lustre. As each film is composed of many separate films, the colours

* *Sur la structure des corps solides*, par M. CH. BRAME (Lettre à M. Babinet). *Comptes Rendus*, &c., Nov. 1853, tom. xxxv. pp. 666, 667.

of the two pencils are those of thin plates, and, as might have been expected, are complementary. Each of the separate films has a colour of its own depending on its thickness; and when they are numerous, the resulting tint is of a very composite character.* The transmitted tints are brightest when the light is incident perpendicularly. They vary at oblique incidences, and disappear wholly when the plate is considerably inclined. Although the elementary films adhere with such force that it is difficult to separate them, they are not, as I have already stated, in optical contact. The plate of air which lies between them exhibits in a very beautiful manner numerous fringes depending on its thickness, as shown in Plate XI. fig. 12. When we apply a drop of water, or alcohol, or oil, to one of the compound films, the fluid advances irregularly between the elementary films, forming a prismatic line of various colours as it proceeds. When the film has been permeated by the fluid, its colours entirely disappear. When the water or the alcohol is evaporated by the application of heat, or dry air at one edge of the wetted film, the colours reappear, and the moving prismatic line separates the dry from the wet portion of the film. The origin of the prismatic fringe is easily explained. If the fluid enters or quits the space between two of the films more quickly than it does between other two, the colour at that place must differ from that of the compound film. The line of the advancing or retiring fluid must therefore consist of several colours depending on the number of elementary films, whose actions are not affected by the fluid.

The following are the colours of the prismatic line, or the succession of changes from the colourless transparency produced by the complete absorption of the alcohol to the original tint of the film:—

Original Tint.	Succession of Changes, &c.
Red.	Pale green, green, blue, pink, dark red.
Rich orange red.	Pale blue, blue, pink, pink nearly opaque.
Yellowish orange.	Greenish, bluish, pink, orange red, deep orange red, paler orange red.
Bright yellow.	Pale pink, bright pink, orange yellow, orange, orange yellow.
Yellowish green.	Pale yellow, dirty yellow, deep green.
Pale bluish green.	Bright green, growing paler and paler.
Deep blue.	Dirty yellow, dirty green, blue, bluish green, blue.
Colourless.	Pale green, brighter green, pale green.

If we use Canada Balsam, or Balsam of Capivi, we sometimes see two waves or prismatic lines, the absorption of the fluid taking place more rapidly between one portion of the elementary films than it does between another portion of the same films. If we use so little balsam that it advances only through half the length of the film, we have the half (A) colourless, and the other half (B) of its original brilliant colour; and if the balsam indurates, the wave of colour which separates the two portions will be permanent. If we drive out some of the balsam from (A) by alcohol, it is curious to see under the microscope the particles of the

* See Phil. Trans., 1837, p. 249.

balsam rushing out in consequence of being displaced by the alcohol. If we allow part of the balsam to remain in (A), its colour will reappear, and be brighter and deeper than that of (B), or its own original colour.

When the surface of any film is specular, or slightly roughened by innumerable cavities, which the microscope is required to discover, or when it is covered with flat polygonal cavities, which rise or sink but slightly beneath the general level, and which are separated by a reticulated structure, namely, the outlines of the polygon, the colour of the plate is uniform, or of one tint; but when the cavities are deep, and especially when they are not very numerous, their colour is not the same as that of the rest of the film. When of the same colour, it is sometimes less and sometimes more intense. In many specimens the deep cavities have a different colour from the rest of the film, as in Plate X. fig. 4, and Plate XI. figs. 6, 7, and 8, the middle or apex of the cavity having often a paler ring round it. Sometimes one side of the cavity has a different colour from the rest of it, which may arise either from its elementary films being more inclined to the incident light, or more numerous.

In a few specimens where the colour is uniform, and consequently the thickness of the film equal, I have observed bands of a different colour, which it is difficult to explain, as the microscope does not show any structure connected with the bands. In some specimens the film is crossed with differently coloured bands and lines extremely narrow and fine, but as they are bounded by sharp edges, they must arise from parallel veins existing in the glass previous to its decomposition. Other films exhibit differently coloured bands, which arise from lines of minute cavities, which, as we shall see, is proved by their action upon polarised light.

In a paper "On the Action of Uncrystallised Films upon Common and Polarised Light,"* I have described the phenomena exhibited by transparent films of decomposed glass, when exposed to these two kinds of light. In polarised light the deep cavities in colourless plates, whether polygonal, spherical, or oval, exhibit distinctly, though with different degrees of brightness, the black cross, with the four luminous sectors of the system of rings seen along the axis of uniaxial crystals. The tints in these sectors never rise above the *white* of the first order.

When the films are coloured, the luminous sectors have frequently the same colour as the film, but in many cases they are different. In a film *orange red* by common transmitted light, the colour of the luminous sectors shown in polarised light is *blue*. In another, *blue* in common light, the luminous sectors are *red*, as in Plate X. fig. 4. In a third, with *green* in the centre of the cavities and *red* round the margin of the cavity in common light, the colour of the luminous sectors is *pale red*. In many other specimens the rings, or circular bands, in cavities traversed

* Edinburgh Transactions, vol. xxii. p. 607.

by the black cross, are very remarkable. The following are some of the most interesting cases which I have observed:—

1. The colour of the sectors is *brownish* near the intersection of the black cross; then come two *green* bands or rings separated by a broad *black* band.

2. *White* near the intersection, then a *black* band, and then a *reddish yellow* one.

3. The order of the bands from the centre or intersection of the black cross is as follows: *green*, *darkish* line, *yellow*, *black*, *brownish*, and the outer band a *bright pure white*.

4. At the centre brownish *red*, then *white*, *green*, *white*, *black*, *reddish*, *black*, *faint light*, *black*, *brownish white*.

5. *Olive green* at the centre, a double *red* band, *olive green*, a broader *red* band, then alternations of *red*, *green*, and *white*, the outermost band being a bright *white*.

These white, dark, and coloured bands, are obviously produced by the interference of pencils that have suffered one or more reflexions with the pencils transmitted by the polarising films.

6. In a remarkable specimen, from which I was able to detach many films, the centre of the figure, or intersection of the black cross, was pale-white, partaking of the colour of the film. This was followed by distinct dark lines, indicating the place where we begin to see through the edges of the hollow spherical surfaces. Beyond this line there were innumerable narrow rings, often of brightly coloured light. This will be understood by examining Plate X. fig. 1, and supposing that the centres of the decomposed cavities have been removed. Through this central part there will be seen only the black cross.

In various specimens of decomposed glass we find films which exhibit in polarised light a number of bright and coloured lines often parallel to one another, and so very small that they require a considerable magnifying power to see them. They are shown, when large, in Plate X. fig. 5, where they obviously are produced by a great number of cavities lying in a straight line. Each cavity produces four luminous sectors, and as the middle of the cavity is occupied with the black cross, the luminous edges of two opposite sectors form the bright lines of polarised light. Upon turning the film round, these lines disappear when they are in the plane of primitive polarisation, and have their brightness a maximum when they are inclined 45° to that plane. When the luminous lines are thus formed, we may generally satisfy ourselves by using high magnifying powers, that they are produced by the edges of cavities; but I have observed bright lines and bands which must have a different origin, and which I have no doubt arise from a structure existing in the glass previous to its decomposition.

In many of the fragments of ancient glass that have obviously been formed

in a mould, the fusion has been so incomplete, that the glass has been drawn out into fibres often curled like a lock of hair, with groups of the fibres twisted in various directions. When a film has been formed by decomposition upon such a surface, different parts of it must be differently inclined to the general surface and to one another, and will polarise the incident light in conformity with the observations in the paper already referred to. When the inclined portions of the film thus produce bands in polarised light, their inclination is finely seen by the reflexion of ordinary light from their surface, which is distinctly fluted as if the film had been formed on a series of rounded surfaces. These cylindrical flutings reflect different colours, depending on the different inclinations of their sides to the incident light.

Among the more interesting phenomena of decomposed glass are the various circular and other crystals, which are frequently found either upon glass slightly decomposed, or between the laminæ of the coloured films. That these crystals are formed by ingredients of the glass cannot be doubted. That they are crystals of silex is highly probable. In films of a pale sky-blue colour, the crystals, both in common and polarised light, are always colourless, a fact not easily explained; but in other films they have the colour of the laminæ which enclose them. These crystals have various forms and structures.

1. They are very frequently circular crystals, consisting of minute crystals radiating from a centre, often with a perfectly circular margin, and of uniform intensity of light. They are sometimes very irregular in their outline, but even in this case the elementary crystals are directed to a centre, so that a distinct black cross separates the luminous branches.

2. The crystals are often so irregularly distributed, that they have no relation to a centre, but form lines and curves of every possible shape.

3. The crystals are frequently crystalline patches, each of which has a neutral and depolarising axis, as if they were fragments of a thin film of a doubly refracting crystal. In some specimens the field of the microscope is filled with hundreds of them, among which a few minute circular crystals may be detected.

4. In several specimens the crystals resemble the leaves of a plant, as shown imperfectly in Plate XI. fig. 9, four or more leaves being arranged round a centre. Occasionally, a second group of these leaves surrounds the first group. A compound crystal of this kind was found to be nearly $\frac{1}{50}$ths of an inch in diameter.

5. Groups more remarkable than these have more than once presented themselves to me. In one of these viewed in the dark field of the polarising microscope, a bright and minute speck occupies the centre. At a certain distance from this point there is a ring of minute bright crystalline specks, the bright point being the centre of the ring. At more than twice the above distance from the centre is a much wider ring of luminous specks, exactly like a ring nebula or cluster of stars,—with this difference, that the middle of the ring is the brightest, the specks

shading off on each side of it. At nearly double the distance of this ring from the centre is a very narrow and well-defined luminous ring, consisting of hundreds of fan-shaped crystalline groups, with their apex directed to the centre of the rings. On the outside of this bright ring, and touching it, are minute groups rounded like the teeth of a wheel. This remarkable phenomenon occurs in a film which is greenish-blue in ordinary reflected light, and pale-pink by transmitted light. Immediately beyond the bright, well-defined ring already mentioned, there is, in ordinary light, a distinct blue ring a little wider than the bright ring; and beyond this is a sort of annular space of a paler pink colour, by transmitted light, than the rest of the film. The diameter of the bright well-defined ring is about $\frac{5}{8}$ths of an inch.

The relation of all these rings of minute crystals to the bright central point, places it beyond a doubt that they are produced by some force emanating from, or connected with, that point. What such a force is, acting between films of glass, and collecting round it innumerable crystalline specks formed during the decomposition of the glass, it would be idle to conjecture. In my paper on Circular Crystals, I have mentioned some analogous phenomena, which may deserve the notice of future observers.*

Many of the phenomena described in the preceding paper will be better understood from the following description of the plates. The drawings were carefully made for me by the Honourable Mrs WARD, from specimens which I sent to her; but, correct and beautiful as they are, they convey but an imperfect idea of the structures which they delineate.

Explanation of Plates X., XI.

Fig. 1. Is drawn from a film of decomposed glass from Nineveh. It shows, as seen by reflected light, the mode in which the decomposition proceeds round one or more centres.

Fig. 2. Shows on a less magnified scale the process of decomposition in glass from Nineveh.
In glass found in the garden of St Leonard's College, St Andrews, black specks arranged in circles seem to be some of the ingredients of the glass in a crystalline form.

Fig. 3. Shows a portion of the film, fig. 12, as seen by polarised light. In this figure the black or dark cross is shown with more or less distinctness in several of the cavities. Whenever the black cross is seen in polarised light, it is finely shaded off, as in the systems of rings in uniaxial crystals.

Fig. 4. Shows, by ordinary transmitted light, the curious phenomenon of numerous small cavities produced by decomposition round numerous centres in larger cavities, the small and the large cavities consisting of numerous spherical films. In every one of the cavities the black cross is distinctly seen. This film, as seen by polarised light, is shown in fig. 6. In the large cavity, at the lower end of the figure, the black cross which it would have

* Edinburgh Transactions, vol. xx. p. 621.

exhibited, had there been no small cavities in it, is not shown even in the imperfect state in which it exists, owing to its eclipse by the smaller cavities.

Fig. 5. Is a film seen by polarised light, and exhibiting the cavities arranged in lines, as described in p. 193. The drawing of this film as seen in common light, was accidentally omitted by the artist. It had the same colour as fig. 12.

Fg. 6. Shows the right-hand part of fig. 4 in polarised light. (See fig. 4.)

Fig. 7. Is another specimen as shown by polarised light, having an elliptical cavity exhibiting a diffused black cross.

Fig. 8. Show groups of crystals of silex formed between the films, in transmitted light.

Figs. 9 and 10 show the same magnified. There are numerous examples of the leaf-like branches shown in fig. 9, and still more numerous examples of the silex occurring in circular crystals of singular beauty and various forms. (See p. 194.)

Fig. 11. Shows a film as seen by reflected light, the *convex* sides of the cavities being towards the eye. (See p. 188.)

Fig. 12. Shows, very imperfectly, in the faint blue lines, the fringes produced by the different thicknesses of the plate or plates of air between the films of glass. See p. 191.

Fig. 13. Shows a film as seen by reflected light, the *concave* sides of the cavities being towards the eye. (See p. 188.)

It is often difficult to determine whether the convex or the concave sides of the cavities are towards the eye, when they are seen by reflected light; but the image of the flame of a candle in the inverting microscope is always on the *right* hand of the cavity when its *concave* side is towards the eye, and *vice versa*.

N.B.—I have used the word cavity to denote each spherical group of films, like a number of watch-glasses placed within one another, the concave side or cavity being supposed to be towards the eye—the optical phenomena seen by transmitted light, whether common or polarised, being the same when the convex sides are towards the eye.

XX.—*On the Polarisation of Light by Rough and White Surfaces.* By Sir David Brewster, K.H., D.C.L., F.R.S.

(Read 2d March 1863.)

The laws of the polarisation of light when reflected from the surfaces of solids and fluids, and when refracted and transmitted by translucent and transparent bodies, have been successfully investigated; but no experiments, I believe, have been made on the polarisation of light by rough or unpolished surfaces, such as ground glass, painted surfaces, pounded glass, snow, white powders, and solids and fluids reflecting white light from their interior. When studying the polarisation of the atmosphere, and anxious to discover the cause of its partial polarisation, and of the three neutral points or spots, in which there is no polarisation, I investigated the action of rough surfaces upon light, under the conviction that the sky or atmosphere was a rough surface like any aggregation of white or coloured particles. Had the atmosphere been specular like water or any body with a polished surface, the image of the sun would have been seen in it by reflexion, but being composed of aerial and aqueous molecules, it must reflect the sun's rays like pounded glass, or any white or coloured powders.

The results of this inquiry, which I now submit to the Society, are such as I anticipated, and afford an explanation not only of the partial polarisation produced by the atmosphere, but of each of the three neutral points, which, it will be shown, can be produced artificially by the combination of rays polarised by the reflexion and refraction of any rough or molecular surface.

The experiments by which these results were obtained were made chiefly with rectangular plates of glass, 9 inches by 7, of various degrees of roughness, some with only one side, and others with both sides rough. These plates, some of which are now on the table, were made at the Smethwick Glass Works, near Birmingham, and were kindly presented to me by Messrs Chance and Brothers, the proprietors of that great establishment.

The angle of complete polarisation for light reflected from the polished surface of this glass is about $56\frac{1}{2}°$, and the polarisation is a maximum at this angle, diminishing on one side to 0° of incidence, and on the other to 90°. When the light thus polarised is examined by the band polariscope, the bands are nowhere interrupted, and therefore there can be no neutral point, the polarisation of the bands being everywhere positive or vertical.

In order to observe the effect of a single rough surface, such as that of glass ground with the finest emery, I blackened with melted wax the polished surface of a plate which was ground only on one side, in order to prevent the light scattered by the ground surface from being reflected at the second surface. When this single rough surface reflected the light of a gas flame, it polarised it almost completely at nearly the polarising angle of $56\frac{1}{2}°$, and there is no interruption or neutral point in the bands of the polariscope. The Rotation, therefore, or measure of polarisation, is nearly 45°.

If this single rough surface is placed at an open window when the sun is not shining, and reflects the light of the sky or clouds, the light is only partially polarised, and the degree of polarisation R is only 19°. In the open air it is much less. Within the room the polarisation increases as the glass recedes from the window. In the open air it is a maximum when the plane of reflexion passes through the sun and the observer. The rotation is then 15°, and it diminishes with the distance of the plane of reflexion from the plane passing through the sun and the observer.*

When the second or polished surface is not blackened, it reflects the light scattered by the ground surface, and the maximum polarisation of the rough surface is greatly diminished. The rotation is 16° with a gas burner very near, and 20° when the flame is still nearer. The polarisation is diminished by holding a lighted candle on one side of the plane of reflexion.

With a similar plate having a rougher surface, the degree of the polarisation of the sun's rays directly reflected was $16\frac{1}{2}°$ close to a south window. Before noon the angle of maximum polarisation was 44°, and at noon, with a brighter sun, it was 40°.

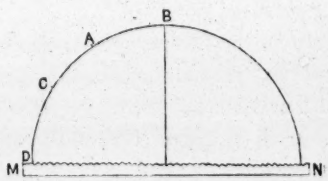


Fig. 1.

When the whole light of the sky fell upon the rough surface MN, the rotation was 21°. When AB was covered up the rotation was diminished, and when CD was covered up the rotation was increased.

When both the surfaces of the plate of glass are rough, the polarisation is a maximum, at an angle greater than the normal polarising angle, or $56\frac{1}{2}°$, and the degree of polarisation is about 20°.

In making similar experiments with ivory, bone, porcelain, white or coloured surfaces painted in oil, paper, parchment, silk, linen, and cotton cloths, milk, flour, and other white powders, &c., I found *that the polarisation was partial* in all of them, the normal or complete polarisation being reduced by its combination with the oppositely polarised rays produced by refraction.

In all these experiments the partial polarisation was positive or vertical from

* The roughness of the glass surface used in the preceding experiments is such, that a gas flame, distant $5\frac{1}{2}$ feet, ceases to be visible at an angle of incidence of 79° 50′.

an incidence of 90° up to an incidence of 0°, as far as it could be observed, owing to the impossibility of examining the bands where they were reflected at or near a perpendicular incidence. In order to meet this difficulty, I adopted the following mode of observation:—

The rough or white suface MN being placed vertically, was illuminated with the flame of a gas burner, or a moderator lamp, placed at F. The observer at E, or in any direction between F and N, observes the condition of the bands of the polariscope when they are placed parallel to MN.

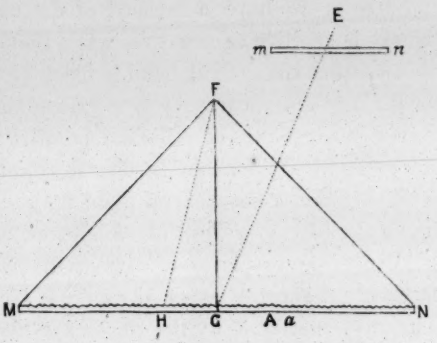


Fig. 2.

If we now take a single rough surface of plate-glass *blackened* on its polished side, and place it at MN, we shall observe a neutral point about H, the polariscope bands being *negative* or *horizontal*, with a *white* centre on the M side of H, and *positive* or *vertical*, with a *black* centre on the N side of H. This neutral point is obviously produced by the equal and opposite action of light polarised by reflexion and refraction. As these two lights, proceeding from light incident at H, proceed from F, they cannot reach the eye at E by the ordinary law of reflexion. They are portions, therefore, of oppositely polarised rays scattered in every direction by the rough surface of the glass.

If we take the same plate of glass with its surface *not blackened*, and place it at MN, we shall find the neutral point at G.

If we invert this plate, so that its polished side is uppermost, the neutral point is advanced to A, all the bands on the M side of A being, as before, *negative*, and those on the N side of A *positive*,—a result proving that the intensity of the negative bands had been *increased* and the positive ones *diminished*.

Conceiving that this effect was produced by the light scattered by the rough surface being polarised *negatively* by the refraction of the polished surface, I took the plate with the blackened side, and laid upon its surface a plate of transparent glass, in order to imitate the action of the unpolished surface in the preceding experiment. The neutral point which, by the action of the single rough surface, was at H, was now advanced to A by the refractive polarisation of the two surfaces of the transparent plate. When this plate was placed at *mn* parallel to MN, the very same effect was produced. By inclining *mn*, the neutral point was advanced from A to *a*, still farther, by increasing the inclination, and still farther, by using several plates, the plane of refraction or incidence being parallel to a plane passing through FGA.

If we turn the plate or plates at *mn* round 90°, keeping the same inclination

to the incident rays from MN, the neutral points will return to their respective places *a*, A, G, H, the refractive polarisation of the plate or plates reducing the negative bands between M and the neutral points, and increasing the positive bands between the neutral points and N.

Hence, we have a method of determining whether the polarisation of the bands is positive or negative, when they are so faint or indistinct that we cannot see whether the central band is black or white. When they become *less intense*, or perhaps disappear, by viewing them through one or more plates of glass at *mn*, having their planes of reflexion parallel to the plane passing through FA, they are *positive*. When they become *more intense* they are *negative*.

The place of any neutral point may be advanced from H to G, or from G to A and *a*, by placing a sheet of *white* paper behind the rough surfaced plate MN, or by one or more plates of glass with rough surfaces, each plate advancing it farther. A sheet of paper advances it farther than *four* surfaces of ground glass. The rays scattered by the paper are negatively polarised by refraction at their emergence from the Plate MN, and, by increasing the intensity of the negative bands, shifts the neutral point towards N.

We have already seen that a single rough surface of glass blackened behind has its neutral point about H, and when not blackened about G. A plate with both sides rough carries the neutral point towards A, and two and more such plates still farther. The smoothest ground plates carry the neutral point farther than the roughest towards M.

In the prosecution of this subject, I submitted to examination the following substances :—

Calcareous spar ground.	Linen cloth, white.	Swan's down.
Marble white.	Cotton cloth, white.	Snow.
Painted board.	Cashmere, white.	Pearl oyster shell.
Ivory.	Paper of all colours.	Pounded sugar.
Ivory artificial.	Parchment.	Pounded glass.
Bone.	White kid leather.	Rochelle salts.
Porcelain.	Pith of the sola.	Soda.
White fir wood planed,	Rice paper.	Magnesia.
Yellow fir wood planed.	Cotton wool.	Milk.
Silk, white.	Ermine.	White shell.
Satin, white.		

In all these substances I found a neutral point in the mixture of the positively and negatively polarised rays which they reflect. In many of them the neutral point was on the A side of G, or when the angle of incidence was less than 90°, while in others it was on the H side, or when the angle of incidence was negative and greater than 0°, but on the other side of the perpendicular FG.

As in ground glass, one or more plates of glass inclined to the reflected ray, as previously described, brought the neutral point, in almost all these substances, to the A or N side of G.

When the substance was more or less glazed, the neutral point was on the A side of G, the glazed surface acting exactly like the polished surface of a plate of ground glass when placed next the light. This effect is finely seen in milk, where the fluid surface increases the negative or refracted light as in the glass plate.

I attempted to determine for several of these substances the angle of maximum polarisation, the intensity of the partial polarisation, and the place of the neutral point; but I found it very difficult, owing to the magnitude of the flame which was necessary to show the bands when very faint, and to its proximity to the reflecting surface, which was necessary from the same cause.

In white unglazed *paper* for example, in the sun's light on the 1st of February 1841, at 11^h A.M., the polarising angle was about 71°, and the degree of polarisation, or R=18¼°. On the 2d February the polarising angle was 69½°, and R=18¼° at $10^h\ 40^m$ A.M.

In almost all the substances which I have examined, I have observed a neutral point only at angles of incidence below the maximum polarising angle, and beyond G, fig. 2; but it is obvious that there must be another at some angle above the maximum polarising angle, excepting in substances where the light polarised by refraction is too feeble to neutralise the light polarised by reflexion. I have observed this second neutral point only in one case, but with a sufficiently strong light it will doubtless be seen in many substances.

In all the preceding experiments, the substances employed have been opaque, or with such rough surfaces, that objects cannot be seen through them. I was therefore desirous of ascertaining if neutral points were produced when the surfaces which polarise the light were perfectly transparent, having some analogy with the strata of the atmosphere of different densities. With this view I used a pile of transparent plates with twenty surfaces, and I found a neutral point distinctly visible, both above and below the angle of complete polarisation. I obtained the same result at an angle less than that of complete polarisation, with a large plate of mica split by an intense heat into so many films that it had the metallic lustre of silver.

The results now submitted to the Society could hardly have been anticipated from theoretical considerations. The laws of polarisation for light, normally reflected and refracted by polished surfaces, are not applicable to those which are rough, or to bodies which reflect light from their interior; and in the case of piles of polished glass-plates, the most distinguished philosophers, ARAGO, YOUNG, and Sir JOHN HERSCHEL, would have considered the light which the plates reflected, as they did that which they transmitted, as consisting of polarised light, accompanied with a portion of common light, a combination incapable of producing neutral points.

The most important application of the preceding experiments is to the polarisation of the atmosphere. ARAGO, BABINET, and others, in their theory of the neutral points, and of the partial polarisation of the atmosphere, took no account of the rays which are polarised by refraction, whenever light is polarised by reflexion, and they referred these abnormal phenomena to a horizontal secondary reflexion from the atmosphere itself, modifying, and, in three neutral points, extinguishing the light polarised by reflexion. Whether or not such a secondary reflexion exists, or is adequate, if it does, to account for these phenomena, are questions which will be considered in another paper on the polarisation of the atmosphere. But however ingenious may be the hypothesis, it has no support either from experiment or observation. The reduction of complete to partial polarisation, by the opposite action of light polarised by reflexion and light polarised by refraction, and the production of neutral points where these two lights are equal, whenever light is incident on surfaces, which, like the atmosphere, disperse and polarise it, is next to an ocular proof of the true laws of atmospherical polarisation.

It is not one of the least wonders of terrestrial physics, that the blue atmosphere which overhangs us, exhibits in the light which it polarises phenomena somewhat analogous to those of crystals with two axes of double refraction.

Fig. 1.

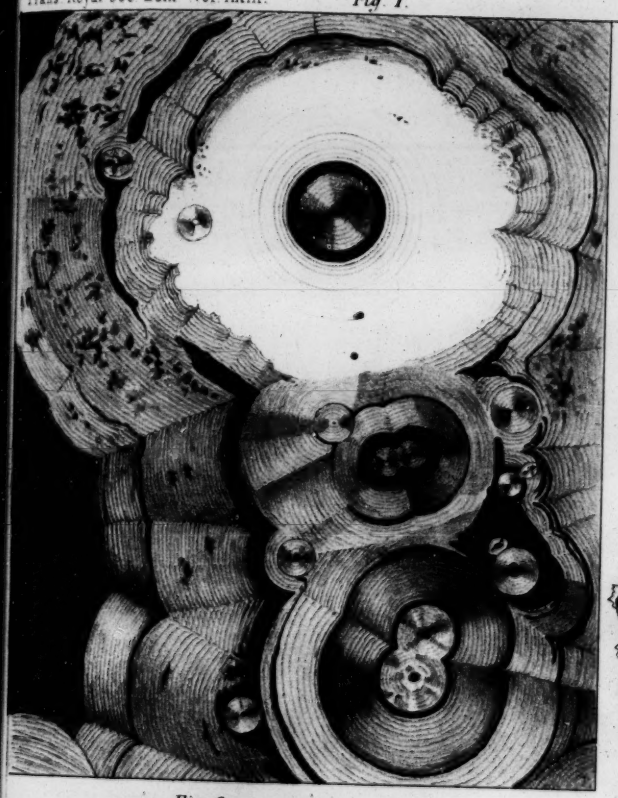


Fig. 3.



Fig. 4.



Fig. 2.



Fig. 5.



W.H. Mc Farlane Lith^{y} Edin^{r}

Fig. 6.



Fig. 7.



Fig. 8.

Fig. 9



Fig. 10.

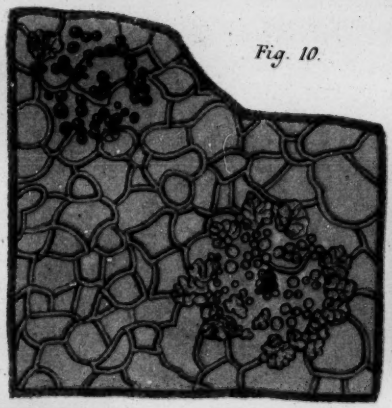


Fig. 11.



Fig 12.

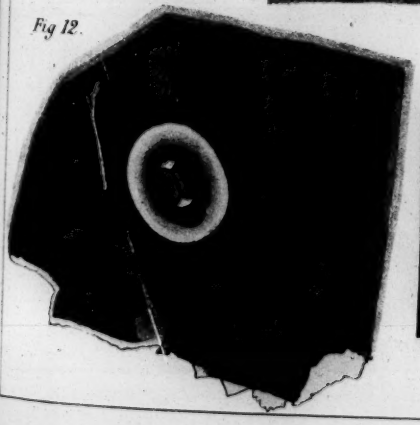
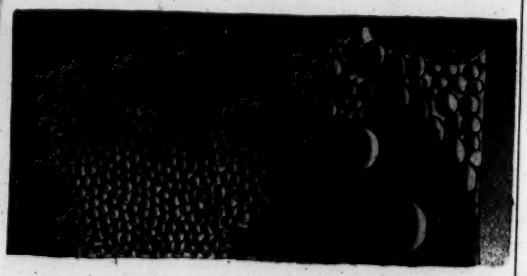


Fig. 13.



W. H. Mc Farlane, Lith.r Edin.r

BRITISH
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XXI.—*Observations on the Polarisation of the Atmosphere, made at St Andrews in* 1841, 1842, 1843, 1844, *and* 1845. By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., &c. (Plate XII.)

(Read 16th March 1863.)

During the last half century, observations on the polarisation of the atmosphere were made by several eminent observers—by ARAGO, DELEZENNE, BABINET, and ZANTEDESCHI; but no result of special importance was obtained till ARAGO made the great discovery that there existed in the atmosphere a point, or spot, in which there is *no polarisation*. At sunrise or sunset he found that this neutral point was 20° or 30° above the point opposite to the sun, or what we may call the *Antisolar point*. The name of *Arago's neutral point* has been given to this spot without polarisation. It is best seen after sunset. At St Andrews it is above the horizon all the day, between the middle of November and the end of January.

In the year 1840, M. BABINET made the next important discovery respecting the polarisation of the atmosphere. When on a visit to the sea-coast, he discovered that there was a neutral point as far above the sun as ARAGO's neutral point was above the antisolar point.* To this spot the name of *Babinet's neutral point* has been given. It is most distinctly seen immediately after sunset, but is much fainter than the other, on account of the discoloration of the sky by the yellow light of the setting sun.

Upon hearing of this discovery, I saw that we had now the elements for determining the laws of the polarisation of the atmosphere; and, being ambitious of succeeding in such an inquiry, I devoted four years to the study of the subject.

My observations commenced on the 28th April 1841; and I made many hundred on the position of the two neutral points,—on their change of place under different states of the weather, different degrees of transparency in the atmosphere, different degrees of light in the sky, and different altitudes of the sun. I measured also the maximum polarisation of the atmosphere in different azimuths between that which passed through the sun and the zenith, and that which, at sunset, passed through the sun and the horizon. These observations

* Comptes Rendus, &c., 1840. Tom. xi. p. 618.

were not difficult to make when the sky was clear and blue; but in studying the part of the atmosphere between the sun and the horizon, I was perplexed beyond measure with the feeble and uncertain indications of the polariscope. The sky between the sun and the horizon is always the most impure portion of it; and the flood of light streaming from the sun unfits the eye for detecting faint traces of colour. Theoretical considerations had led me to believe that a neutral point should be found between the sun and the horizon, and certain indications of the polariscope, at places around its probable locality, entitled me to infer that it did exist; but, as an inference was not satisfactory, I watched every favourable state of the sky, in the hope of obtaining a more direct result.

An opportunity of doing this at last presented itself to me on the 28th February 1842, when the sun was in the meridian, with an altitude of 22°. The spot beneath the sun was fortunately vibsile from the end of a long dark passage running north and south, and having concealed the sun himself, and every part around him except the probable position of the spot, I obtained a most distinct view of the new neutral point, situated about 15° or 16° below the sun. After communicating this discovery to M. Babinet early in 1845, he made several ineffectual attempts to confirm it, and it was not till the 23d July 1846 that he succeeded in obtaining a distinct view of this neutral point. Following the usual practice, the French have given to this spot the name of *Brewster's neutral point.*

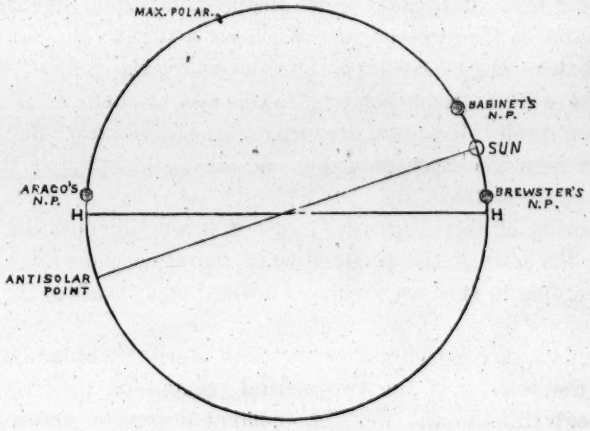


Fig. 1.

With these three elements of atmospherical polarisation, shown in Fig. 1, I shall now proceed to determine their position in relation to the sun and the antisolar point, and ascertain the changes which they undergo from variations in the optical and meteorological state of the atmosphere.

Observations on ARAGO'S *Neutral Point.*

In observing the different neutral points, I employed chiefly SAVART'S band polariscope, which was kindly presented to me by M. BABINET. It consists of two plates of rock crystal, which give a system of rectilineal polarised bands* attached to an analysing plate of tourmaline. In very feeble lights, I used another instrument, in which the analyser was a NICOL'S prism. When the polariscope is directed to a neutral point, the system of fringes is interrupted, as shown in Fig. 2 at N. P. The bands on one side of the interruption are oppositely polarised to those on the other, the central band in the one being *black*, and in the other *white*.

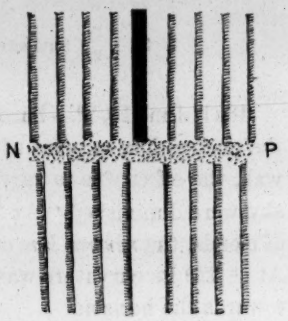


Fig. 2.

In order to measure the altitude of the neutral point thus observed, I used a common quadrant with a plumb line, which gave, within half a degree, the altitude of the centre of the neutral spot. In order to obtain the height of the neutral point above the sun, or the antisolar point, it was necessary to have the sun's altitude, or his depression beneath the horizon. Having asked Professor HENDERSON for the easiest method of obtaining these with a moderate degree of accuracy, he recommended to me to use MARGETT'S Longitude Tables, from which they were accordingly taken.

The following observations are a selection of some of the most important, out of a very great number recorded in my journal. In order not to crowd the page with figures, I have given only the hour of observation, and the altitude of the neutral point above the antisolar point, and occasionally the maximum polarisation.†

1841, May 12.—Barom. 30·1; Therm. 9^h P.M. 48°. The sky unusually clear.

Mean Time.		R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
7^h 10^m P.M.		30½°	28½°	20° 33′
7 25 „		...	...	20 29
7 35 „		30½	29½	21 13
7 45 „		28½	28½	21 33
7 55 „	The sun set.	...	...	22 8
8 5 „		...	...	21 35
8 15 „		...	...	20 41
8 30 „	The polarized bands faint.		...	22 26

* See Edinburgh Transactions, 1819, vol. ix. p. 148, where the method of producing rectilineal bands, by crossing two plates of rock crystal, was first published.

† See the section on the place of maximum polarisation, and its intensity or R., p. 245.

1841, June 3.

Mean Time.		Height of Neutral Point above the Antisolar Point.
6h 27m P.M.	Neutral Point not risen.	
6 36 „	... very near the horizon.	
6 43 „	... above and close to the horizon.	
7 8 „		18° 20′
7 35 „		20 30
8 4 „		21 43
8 33 „	Sun sets.	

1841, June 10, 2h.—Barom. 29·7. Singularly fine sky all day; wind east. When the neutral line or point of the polarimeter happened to be above an illuminated wall, the effect was to vary the place of the neutral, as if the polarisation of the sky were diminished. At 3h 47m, when the sky was everywhere pure and free of clouds, the *neutral line* of the polarimeter was curved above the land horizon. At 6h 32m its curvature was greater above the sea horizon, the deviation increasing towards the horizon.

Mean Time		R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
6h 35m P.M.	Neutral point in horizon.			
7 13 „		...	...	18° 20′
7 30 „		28½	28½	18 56
7 42 „		...	...	21 0
8 5 „		29½	28½	19 15
8 18 „		...	29½	18 56
8 36 „	Sun set.	...	...	19 25
8 52 „		29½	30	19 29

1841, July 17.—Barom. 29·60, therm. 55°. Wind south-west. A China-ink sky, with clouds here and there.

Mean Time.		Height of Neutral Point above the Antisolar Point.
7h 35m P.M.		19° 0′
8 0 „		17 48
8 18 „	Sun sets.	17 25
8 30 „		17 45
8 47 „		17 19
9 2 „		16 15
9 12 „		17 26
9 22 „		19 7

1841, August 31.—Rain till 1h P.M., when it cleared up. Barom. 29·7.

Mean Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
5h 44m P.M.	27½°	18½°	17° 22′
6 0 „	28½	22½	19 55
6 20 „	...	...	19 59
6 38 „	...	...	20 3

1841, September 6.—Barom. 29·5.

Mean Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
6h 22m P.M.	29½°	...	19° 7′
6 30 „	...	...	19 45
6 35 „	28½	27½	20 21
6 49 „	28½	26½	21 20
7 0 „	...	...	21 0
7 6 „	...	...	21 2
7 15 „	...	...	23 0

1841, September 12.—Barom. 29·75 Sky clear.

Mean Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
6h 8m P.M.	27½°	25½°	20° 35′
6 20 „	...	...	20 45
6 24 „	...	...	21 7
6 46 „	...	...	22 2
6 49 „	...	...	22 10
6 58 „	...	...	23 54

1841, September 29.—Barom. 28·73 after rain. Sky clear.

Mean Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
4h 23′ P.M.	37½°	24½°	28° 10′
5 24 „	29½	29½	21 35
5 31 „	30	...	21 0
6 2 „	...	...	23 51
6 12 „	...	...	24 15

At 5h 24m the sky was clear, but whitish, and not blue. A little cloud below the neutral point.

1841, October 23.—Rainy day. Cleared up at 3h P.M.

Mean Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
4h 18m P.M.	...	...	21° 5′
4 24 „	...	...	21 50
4 29 „	27½	25½	21 27
4 37 „	...	...	21 13
4 44 „	...	...	21 23
4 52 „	26	25½	21 45

1841, November 2.—Barom. 30·7. Fine day; dry haze.

Mean Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
2h 30m P.M.	The neutral point rises.		
2 30 „	26½°	22½°	13° 20′
2 43 „	...	...	18 20
3 22 „	25	22½	18 45
3 51 „	...	...	21 32
4 15 „	...	...	19 54
4 36 „	...	...	19 20
4 58 „	...	...	22 22

1841, November 4.—Barom. 30·2. A foggy day; sky tolerably clear of clouds.

$1^h\ 50^m$ The neutral point just risen above the thick haze near the north horizon. The bands of the polariscope are all rugged at the edges, indicating an abnormal state of the air. R = 21½° in zenith, and 15° in horizon.

Mean Time.	Height of Neutral Point above the Antisolar Time.
$3^h\ 13^m$ P.M.	24° 52′

This singular height of the neutral point arose doubtless from the haze in the horizon.

Mean Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$3^h\ 45^m$ P.M.	22½°	19½′	...
4 12 ,,	...	...	19° 56′
4 17 ,,	...	...	20 3

1841, November 25.—Barom. 29·63. Singularly fine day.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$10^h\ 31^m$ A.M.	27½°	25½′	14° 30′
12 0 ,,	27½	25½	14 6
2 21 ,,	28½	26½	17 35
3 3 ,,	...	...	18 45
4 20 ,,	...	...	19 50
4 24 ,,	...	...	18 40
4 38 ,,	27½	27½	19 20

1842, January 29.—Barom. 29·93. Fine day; clear sky; snow covers the ground partially.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$3^h\ 34^m$ A.M.	27°	27½°	21° 46′
3 52 ,,	...	...	20 38
4 10 ,,	...	...	20 40

1842, February 15.—Rain in morning; fine day afterwards; Barom. 30·05; wind west.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$3^h\ 13^m$ A.M.	26½°	18½° in S. H. / 22½ in W. H.	Neutral point not risen.
3 38 ,,	...	...	In App. H. 7° 30
3 48 ,,	26½	18½	8 17
3 58 ,,	...	...	9 7
4 21 ,,	...	...	14 35
4 47 ,,	...	...	16 55
4 51 ,,	27½	22½ in S.E.	17 52

The observations at $3^h\ 38^m$, 48^m, and 58^m, are recorded as good, and made in the finest sky.

The state of the atmosphere was peculiar near the horizon, as will be seen in the section on a secondary neutral point.

1842, February 16.—Barom. 30·16.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
12h 0m A.M.	A China-ink muddy sky.	...	...
2 48 „	19½°	...	Neut. Point not risen.
3 18 „	22½	22½ at 25° Alt..	Do. do.
3 22 „	...	...	10° 30′
3 44 „	A secondary neutral point.	...	12 0
4 12 „	...	...	17 30
4 37 „	...	...	17 10
4 58 „	...	...	18 20

1842, February 18.—Cold, wind west and rather strong. 12h 0m, I observed a curious effect on the polarised bands in the west, the sun's altitude being about 30°. In carrying the bands vertically round, the neutral line, in place of crossing them at a right angle, was the arch of a circle, to which one of the bands was a tangent. The sky was clear, but in a short time a cloud was formed in that place. I had observed the same phenomenon previously. It indicates a state of the air similar to that which produces mirage.

1842, February 21.—Barom. 29·44. Wind west. The sky-line over both the sea and the land unusually distinct and free from haze.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
3h 45m P.M.	10° 29′
3 57 „	12 39
4 31 „	17 40
4 37 „	18 35
5 6 „	19 49

1842, April 5.—Barom. 30·07. Splendid sky.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
5h 25m P.M.	...	...	13° 26′
5 32 „	...	...	15 25
6 21 „	30	26½	19 1
6 55 „	...	...	18 50
7 12 „	30½	...	21 5

N.B.—This and April 8th are the only days on which I was able to observe all the three neutral points, and determine their place. (See pp. 225 and 229.)

1842, April 6.—Barom. 30·05. Considerable haze.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
5h 23m	15° 26′

1842, April 8.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
5h 30m P.M.	...	...	15° 26
5 38 „	...	...	19 0
5 46 „	...	...	19 0
5 52 „	...	...	19 5
6 5 „	24½	19½	21 25
6 22 „	...	...	20 55
6 47 „	27½	...	20 30
7 5 „	28¾	...	19 55
7 18	...	...	20 15

1842, April 13.—Barom. 30·12. Fine day.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
5h 48m P.M.	...	...	16° 20′
6 20 ,,	29	22½	17 55
6 54 ,,	29½	...	19 40
7 10 ,,	...	...	19 45
7 19 ,,	30½	...	19 4
7 54 ,,	...	...	22 10

At 7h 32m the maximum polarisation was 32½, the greatest ever observed. (See p. 250.)

1842, April 20.—Barom. 30·02. Wind west; very fine day.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
5h 50m P.M.	22° 50′
6 29 ,,	19 21
6 59 ,,	20 55
7 45 ,,	20 10
7 59 ,,	24 8

1842, April 26.—Barom. 30·00. Not a cloud in the sky from morning till night.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
5h 21m P.M.	13′ 43′
6 6 ,,	13 51
6 38 ,,	19 28
7 9 ,,	20 10
7 32 ,,	19 15

1842, April 29.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
5h 54m P.M.	18° 4′
6 1 ,,	18 35
6 45 ,,	22 25

1842, May 15.—A haze.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
6h 29m P.M.	15°	15°	28° 35′
6 46 ,,	...	...	28 12
7 26 ,,	20½	...	24 6
8 26 ,,	23½	...	19 59

1842, November 14.—Barom. 29·6. Fine frosty and clear morning.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
8h 55m A.M.	25½°	19½°	19° 50′
9 3 ,,	...	...	18 40
9 16 ,,	...	...	15 22
9 31 ,,	...	...	15 0
9 43 ,,	...	...	14 30
9 48 ,,	...	...	13 45

1842, November 20.—Barom. 29·74. Cold and clear.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
10h 8m A.M.	21½°	...	18° 5′
10 15 „	...	...	17 30
10 27 „	...	...	16 25
10 39 „	...	...	45 15
10 55 „	...	...	15 5
11 0 „	Neutral point lingering in the horizon.		

1842, November 21.—Barom. 29·77; therm. 31°. Frosty morning.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
9h 1m A.M.	...	...	21° 10′
9 13 „	26½°	19½°	20 25
11 4 „	...	...	15 45
12 22 P.M.	Neutral point below land horizon; bands scarcely visible in horizon.		
12 43 „	Neutral point in horizon.		
1 40 „			16 30

The decrease in these numbers as the sun's altitude increased is very interesting. The light of the sky was increasing till noon, whereas, when the numbers increase, the light of the sky diminishes.

The observations on the 14th, 20th, and 21st Nov. were the only *morning* ones I made.

1842, December 28.—Barom. 29·56. Sky very fine at 11h 38m.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
11h 38m P.M.	29°	...	17° 20′
11 58 „	27	18½	17 21
1 0 A.M.	...	...	18 25
1 28 „	...	...	22 16
2 33 „	14½	...	22 15

See in p. 237 the state of BABINET's neutral point at this date, when a halo of 45° encircled the sun.

1843, February 2.—Barom. 29·05. Snow storm with wind.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
9h 55m A.M.	24½°	17½°	...
	Polarisation of the sun's light by the snow hardly perceptible, whether we look towards or from the sun.		...
2 5 P.M.	Neutral point in sea horizon.	...	12° 5′
2 47 „	26½	19½	14 35

1843, February 14.—Barom. 29·45. Bitter cold day; frost in the morning.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
2h 48m A.M.	Neutral point below horizon.	...	12° 5′
2 53 „	Neut. pt. in horizon 28½	23½	11 45

1843, March 25.—Barom. 29·97.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
4h 35m A.M.	25° Neutral point in horizon.		10° 0′
5 15 „	...	...	11 30
5 44 „	28	...	18 31
6 9 „	29½	...	17 30

1843, June 21.—Barom. 29·75. Fine day; wind west.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
7h 12m A.M.	27½°	22½° { N. pt. in horizon }	9° 40½′
7 44 „	...	...	16° 31
8 30 „	29	...	19 22
9 16 „	30	...	18 29

1844, February 3.—Barom. 29·90. Snow covering the ground.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
4h 7m A.M.	26°	23°	18° 20′

1844, February 21.—Barom. 29·28. Snow partially covering the ground.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height in Neutral Point above the Antisolar Point.
3h 30m A.M.	27°	...	13° 3′
4 40 „	28½	...	12 35
4 13 „	28¾	...	17 34
4 40 „	...	...	19 24

1844, June 10.—Barom. 29·70.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
7h 36m A.M.	...	...	21° 20′
7 44 „	...	...	21 20
8 11 „	24¾	22½	21 35
8 52 „	27½	23½	21 50

1844, June 13.—Barom. 29·4. Windy; wind south-west.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
7h 0m A.M.	Neutral point in horizon.	...	11° 10′
7 12 „	24½	20½	13 2

In the following observations, the altitude of the sun was not estimated. The numbers in the fourth column are the altitudes of the neutral point above the horizon.

1845, April 15.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Horizon.
5h 48m A.M.	26½°	22½°	7° 25′
6 48 „	27½	24½	18 55

In the normal state of the atmosphere, as represented in the Map, Plate XII., namely, when the sun is in the horizon, ARAGO's neutral point is about $18\frac{1}{2}^\circ$ above the horizon, or above the antisolar point; but when the sun is 11° or 12° above the horizon, and the antisolar point as much below it, the neutral point is in the horizon, and consequently only 11° or 12° above the antisolar point.*

As the sun descends to the horizon, and the antisolar point rises, the distance of the neutral point from the latter gradually increases; and when the sun reaches the horizon, the neutral point is $18\frac{1}{2}^\circ$ above it, and therefore $18\frac{1}{2}^\circ$ distant from the antisolar point.

After the sun has set, the distance of the neutral point from the antisolar point increases; that is, it rises faster than the sun descends, and its maximum distance, when the twilight is very faint, is about 25°.

When the sun is advancing to the meridian, and the light of the sky is increasing, the distance of the neutral point from the antisolar point diminishes, as shown in the *morning* observations on the 14th, 20th, and 21st November 1842.

On a Secondary Neutral Point accompanying ARAGO'S *Neutral Point.*

When the sea horizon was terminated by a dark purple belt about $1\frac{1}{2}^\circ$ above it, I observed that the vertical bands of the polariscope became brighter over that belt.

The same phenomenon was seen, but less distinctly, over the land horizon. It was difficult to measure the amount of this new polarising influence, but it was obvious that we should observe it separately when the neutral point came above the belt. In this case, it would eclipse, as it were, the neutral point, which would recover itself when it emerged from the belt. It was obvious also, that when the negative or oppositely polarised bands came over the belt, the new polarising influence would extinguish them where they had the same polarising force, and form a *secondary neutral* point, the primary one being then out of the belt.

On the 8th of June 1841, at $5^h\ 50^m$ P.M., when the polarised bands were strongest, both on the land and sea horizon, I watched the rise of the neutral point, which, as I had foreseen, *did not appear first in the horizon, but about* $1\frac{1}{2}^\circ$ *above it*, the compensation taking place where the vertical or positive polarisation was weaker than in the horizon. We had now the singular phenomenon of a neutral point with positive polarisation on each side of it. When this phenomenon was more fully developed under a favourable state of the horizon, the positive

* In abnormal circumstances, sometimes only 7°, 8°, 9°, or 10°, as in 1842, February 15 and 16.

polarisation was overcome by the advancing negative polarisation. The negative polarisation was then immediately below the ascending neutral point; but at a certain distance (a few degrees below the neutral point), the negative polarisation was compensated by the excess of positive polarisation close to the horizon, and the beautiful phenomenon was seen of two neutral points, a primary and a secondary, separated by bands of negative polarisation, as shown in the annexed figure.

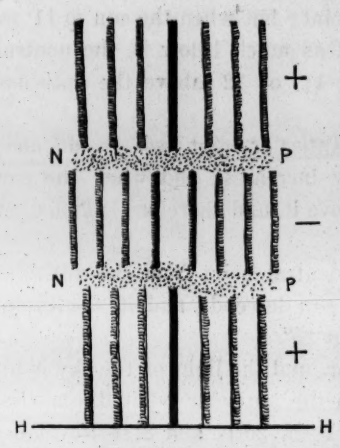


Fig. 3.

1841, June 10, $6^h\ 40^m$.—The neutral point a little above the horizon, with vertical or + polarisation *on both sides of it.* The new vertical polarisation had more than compensated the horizontal, or negative polarisation, and left a balance of positive polarisation, which soon disappeared when the rising horizontal polarisation overpowered it.

1842, Feb. 22.—Both on this day and on the 13th, the neutral point was above the horizon, though not visible, being eclipsed or masked by the cause which produces the secondary neutral point. Over a space of $3\frac{1}{2}°$ above the sea, the positive bands almost wholly disappear before the negative bands are perceptible, and the neutral point is 5° high when the secondary neutral point is distinct in the sea horizon.

Although I have observed the secondary neutral point more than twenty-two times, it has generally appeared under slightly different forms, varying with the intensity of the new polarising cause which produces it, and with the point of the horizon where the neutral point rises.

It is unnecessary to describe these different forms,—I shall mention only an observation made on the 21st April 1849, under very favourable circumstances. At $6^h\ 22^m$, when the primary neutral point was about 15° high, the secondary neutral point was 2° 50′ high, the negative bands covering a space of 8° or 9° between them, the positive bands being above the sea-line. A fog prevailed to some extent, and above the sea-line there was the dark purplish belt previously mentioned, over which the positive bands were stronger than on the part of the sky above it.

Observations on BABINET'S *Neutral Point.*

In the year 1840, when M. BABINET had occasion to visit the sea coast, he proposed to observe if the neutral point of ARAGO varied in its height as the sun rose or set, and to observe it also when the sun was beneath the horizon; but he was allured from these observations by a circumstance which he had never even suspected, namely, the existence of a *second neutral point* above the setting sun,

and nearly as high in the atmosphere as the neutral point of Arago. "I afterwards,"* he says, "determined, a great number of times, the position of this new neutral point which appeared in the west, even when the sun was just on the horizon before setting, and in the east when he had risen only a few degrees. A very imperfect estimate made me sometimes believe, that this new neutral point was a little less high than that of Arago."

The following observations on this neutral point were made generally on the same day, and even in the same hour, as those on Arago's neutral point, and therefore under the same atmospherical influences.

1841, May 12.—Wind west in the evening.

Mean Time.	Height of Neutral Point above the Antisolar Point.
8h 10m P.M.	16° 4′

1841, May 16.—Barom. 29·24. Windy.

Mean Time.	Height of Neutral Point above the Antisolar Point.
7h 0m P.M.	8½°

1841, June 10.—Barom. 29·27. Very fine sky.

Mean Time.		Height of Neutral Point above the Antisolar Point.
10h 10m A.M.	Sun's altitude about 50°.	2° or 3°
7 17 P.M.		13° 15′
7 35 ,,		14 55
7 52 ,,		14 5
8 8 ,,		16 37
8 21 ,,	Sun sets.	15 27
8 39 ,,		17 15
8 52 ,,		16 25
9 3 ,,		16 42
9 15 ,,		16 20

1841, September 6.—Barom. 29·55.

Mean Time.	Height of Neutral Point above the Antisolar Point.
6h 30m	16° 15′
6 45	14 45
6 58	12 55
7 6	13 8

1841, September 12.—Barom. 29·75. Sky clear.

Mean T me.	Height of Neutral Point above the Antisolar Point.
6h 54m	18° 53′
7 0	18 20

1841, September 29.—Barom. 28·73. After rain sky clear.

Mean Time.	Height of Neutral Point above the Antisolar Point.
5h 46m	12° 53′
5 57	14 55
6 7	16 15

* Comptes Rendus, &c., 1840, tom. xi. p. 619.

1841, October 23.—Rainy day; cleared up at 3h P.M.

Mean Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
4h 21m	...	...	17° 33′
27	27½°	25½°	15 35
30	...	...	14 30
39	...	...	15 13
45	26	25½	14 22
53	...	...	15 53

1841, November 2.

Mean Time.	Height of Neutral Point above the Antisolar Point.
4h 50m	14° 51′

1841, November 4.—Barom. 30·2. Foggy day; sky tolerably free from clouds.

Mean Time.	Height of Neutral Point above the Antisolar Point.
3h 13m	14° 4′
4 9	19 0
4 14	17 48

1841, November 25.—Barom. 29·63. Splendid day.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
10h 31m P.M.	...	...	3° 20′
12 9 „	27½°	25½°	6 24
12 26 „	...	...	12 5
2 24 „	28½	26½	13 47
3 10 „	...	...	16 35

1842, January 29.—Barom. 29·93. Fine day; clear sky; snow covers the ground partially.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
3h 32m	27°	21½°	16° 35′
3 53	...	...	17 38
4 8	...	...	17 51

1842, February 15.—Rain in morning, then a fine day.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
4h 25m	...	...	21° 58′
4 44	...	...	20 24
4 55	27½°	22½° in S.E. Hor.	20 30

At this hour clouds rose in the S., N, and N.E. horizon; a dark band of distant haze 6° or 8° high rose above the sea horizon.

1842, February 21.—Fine day; wind west.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
4h 35m	18° 57′
5 8	18 32

1842, March 2.—A wet day; the place of the sun was seen only as a white spot. The polarisation everywhere feeble.

$2^h\ 20^m$.—The neutral point was 75° above the horizon, or about 54° above the sun. The polarisation was negative from the neutral point to the horizon beneath the sun, and positive to the horizon opposite to the sun.

1842, March 16.—Barom. 29·96. The sun occasionally shining through a thickish haze in a China-ink sky without any blue; the wind in the south-west, and slight.

$10^h\ 45^m$.—Sun's altitude about 30½°; the polarisation below the sun was negative down to the horizon; the neutral point was 30° above the sun, or more than 60° high!

1842, April 5.—Barom. 30·07. Splendid sky.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$6^h\ 23^m$	30°	26½°	19° 17′
6 58	...	...	18 46
7 7	30½	...	15 45

The polarisation was unusually great, equivalent to a rotation of the plane of polarisation of about 29°; it became stronger as it grew darker.

N.B.—This day and April 8th were the only days on which I was able to observe the three neutral points, and determine their place. (See pp. 217 and 229.)

1842, April 8.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$6^h\ 0^m$	24½°	18½°	17° 15′
6 25	...	...	18 20
6 45	27½	...	19 11
7 7	28¾	...	21 5
7 20	...	...	19 31

1842, April 13.—Barom. 30·12. Fine day.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$5^h\ 51^m$	...	...	18° 50′
6 23	29°	22½°	18 0
6 58	29½	...	18 40
7 12	...	...	17 35
7 21	30½	...	17 57

At $7^h\ 32^m$, the maximum polarisation or rotation was 32½°, the greatest ever observed. (See p. 235.)

1842, May 15.—A haze.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$6^h\ 34^m$	15°	15°	18° 10′
6 48	...	...	19 55
7 28	20½	...	17 44
8 29	23½	...	18 1

1842, November 14.—Barom 29·6. Fine frosty and clear morning.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
8h 58m A.M.	25½°	19½°	14° 7′

1842, November 21.—Barom. 29·77. Frosty morning. Thermometer 31°.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
9h 4m A.M.	18 36′

1842, December 28.—Barom. 29·56.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
11h 40m A.M.	29°	...	13° 41′
12 3 „	27	18½°	13 37
1 4 „	...	...	25 25!
1 23 P.M.	...	...	27 30!
2 31 „	14½	...	27 48!

These remarkable results were doubtless owing to the causes which produced the following phenomenon:—At 1h 4m, when the neutral point started away from the sun, a white halo of 45° appeared round the sun, and continued till 2h 31m. It was slightly brown on its inner rim. At 1h 23m the altitude of the halo was 32° 10′, and the sun's altitude 8° 30′, so that the radius of its outer rim was 23° 40′.

When the vertical bands of the polariscope passed over the apex of the halo, *their intensity* was increased, and when they passed over the halo in a direction parallel to its horizontal diameter their intensity was diminished. As the crystals of ice, by which the halo was produced, are doubly refracting, one of the pencils must have been weaker than the other, an effect which would be produced if the surfaces of the crystals were not perfectly smooth.*

The polarisation of the sky was greatly reduced by the crystals of ice floating in the air, which produced the halo.

1843, March 25.—Barom. 29·97.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
5h 46m	28°	...	18° 25′
6 11	29½	...	17 41

1843, April 17.—Barom. 29·84. Fine day; wind east.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
6h 33m	17° 32′
7 7	18 4
7 33	18 44

1843, April 29.—Barom. 29·63. Wind east. The heat of the rising sun very great, and the vapour rising copiously from the ground. Everything more than

* See Phil. Trans., 1819, p. 146.

a quarter of a mile distant invisible. The sun shone occasionally, showing his pale white disc.

Apparent Time.	
$10^h\ 20^m$ A.M.	The bands were negative from the zenith through the sun to the horizon, and positive to the opposite horizon. The maximum polarisation was 90° from the sun, but was so weak that it was compensated by the refraction of a plate of ground glass, at an angle of incidence of about 30°, the negative bands being then scarcely visible.
$2^h\ 55^m$.	The vapour still rising copiously, and the place of the sun not visible. The bands were hardly visible, the polarisation, when a maximum, being compensated by the refraction of a plate of glass, at an angle of 5°.

1843, May 3.—Barom. 29·65. Easterly haur.

Apparent Time.	
$4^h\ 25^m$.	Mist flying before the sun, and the neutral point oscillating from near the sun to the zenith as the mist thickens.

1843, June 21.—Barom. 29·75. Fine day; wind west.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$7^h\ 46^m$	...	...	15° 55′
8 33	29°	...	17 57
9 14	30	...	16 28

1844, Feb. 3.—Snow covering the ground.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$4^h\ 11^m$	26°	23°	17° 55′

1844, April 15.

Apparent Time.	Height of Neutral Point above the Antisolar Point.
$5^h\ 54^m$	25° 6′
6 50	18 10

1844, June 10.—Barom. 29·70.

Apparent Time.	R=Maximum Polarisation. In Zenith.	In Horizon.	Height of Neutral Point above the Antisolar Point.
$8^h\ 20^m$	$24\frac{3}{4}$	...	18° 14′

In a normal state of the sky, when the sun is rising or setting in a fine day without clouds, the neutral point of Babinet is situated about 18° 30′ above the sun. Owing to the great quantity of light in the neighbourhood of the sun, this neutral point is not so easily seen as that of Arago, and escaped the scrutiny of that distinguished observer. In high latitudes it is above the horizon the greater part of the year, and, being above the sun, it is of course always visible in a clear sky, when he is above the horizon. When the sun is in the zenith, this neutral point coincides with the sun's centre, its distance from the sun gradually increasing till it becomes 18° 30′ at sunrise or sunset, when the sun's altitude is nothing.

Like that of Arago, the neutral point of Babinet *must* be accompanied, in certain states of the horizontal sky, with a secondary neutral point, but I have never had an opportunity of observing it.

Observations on the Neutral Point below the Sun.

This neutral point, which I discovered in 1842, is much more difficult to be seen than that of BABINET. In November, December, and January, it cannot be seen in our latitude, unless when, early in November and late in January, a high degree of polarisation in the sky brings it above the horizon at noon.

The following interesting remarks of M. BABINET on his successful attempt to confirm the existence of this neutral point, explain in the clearest manner the causes of the difficulties which were experienced, and which every future observer will experience in observing this remarkable spot, with its surrounding polarisations:—

"On the 23d July," says M. BABINET, "after having observed from half-hour to half-hour the polarisation of the sky below the sun, the regularity of this polarisation appeared to change after 4^h; and from $4\frac{3}{4}^h$ to $5\frac{1}{4}^h$ I observed in placing the bands horizontally—

"1*st*, A space without polarisation below the sun.

"2*d*, Below this space a second space, when the bands were certainly seen; and,

"3*d*, Lower still a neutral space, where no bands were seen; and,

"4*th*, In approaching to the horizon, a fourth space, where the bands were very visible. The phenomenon is therefore no longer doubtful; but the immense brightness of the sun in a clear day, the intense illumination of the atmosphere in the region immediately below him, and the reflexion from the strongly illuminated earth, all concurred in rendering this observation difficult to make, and very painful to the eyes, even if we take the precaution of shielding the head and the polariscope from the direct rays of the sun, and the reflexion from the earth. . . . . M. BREWSTER was doubtless guided in his research by theoretical views, otherwise it appears to me very improbable that, by merely observing the atmospherical polarisation, he could have made the remarkable discovery of this neutral point, so difficult to see, and which, after him, I have several times tried in vain to rediscover. . . . . The small quantity of polarised light which is observed between the neutral point of BREWSTER and the Sun, seems to me to reach the limit which it is possible to observe, and perhaps to exceed the limit which it is possible to measure."*

1841, Nov. 17.—Barom. 29·43.

Apparent Time.

$12^h\ 0^m$. The polarisation between the sun and the horizon decidedly *negative*, but no neutral point there.

* Comptes Rendus, &c., 1846, tom. xxiii. p. 234.

1842, Feb. 16.—Barom. 30·16.

Apparent Time.

12h 0m. Sun's altitude about 21° 13′; a China-ink muddy sky. There is clearly a neutral space below the sun, and a little above the horizon. The bands on the sun and below him are negative; but as the negative polarisation becomes very weak, it must pass into a neutral point.

1842, Feb. 18.—Cold, wind west, and rather stormy.

Apparent Time.

12h 0m. The neutral point below the sun was distinctly seen, the polarised bands being negative over and below the sun, and below the neutral point positive down to the horizon. The sun's altitude was about 22°, and the distance of the neutral point from the sun about 15°.

1842, Feb. 21.—Barom. 20·44. Fine dry day; wind west.

Apparent Time.		Distance of Neutral Point from Sun.
12h 39m.	Neutral point 6½° above the horizon,	15° 0′

The positive polarisation between the neutral point and the horizon was compensated by the refraction of *one* plate of glass, at 20° of incidence. The neutral line was convex towards the sun in the west horizon.

1842, March 10.—Sky clear in zenith; wind west.

Apparent Time.

11h 15m. The neutral point distinctly seen below the sun.

1842, April 3.—Barom. 29·8. Fine clear sky; cold; hail in the afternoon.

Apparent Time.

11h 45m. Neutral point below the sun very distinctly seen, the sun's altitude being about 39½°, and the height of the neutral point 26½°. The distance of the neutral point from the sun was 13°. The bands over the sun down to the neutral point were negative, and those below it down to the horizon positive.

1842, April 5.—Barom. 30·07. Splendid sky.

Apparent Time.		Distance of Neutral Point from Sun.
12h 27m.	Neutral point seen distinctly below the sun.	
12 27		15° 25′
12 53		14 40
12 56		15 45
4 13		15 35
4 33		15 22

N.B.—All the three neutral points were observed, and their places ascertained this day, and also on April 8. (See pages 217 and 225.)

1842, April 6.—Barom. 30·05. Considerable haze.

Apparent Time.		Distance of Neutral Point from Sun.
8h 51m.	Neutral point distinctly seen below the sun.	
11 6		12° 0′

1842, April 8.

Apparent Time.

2h 7m. The neutral point below the sun beautifully seen. Estimated distance from the sun, 16° 0′

1842, April 15 and 17.

Apparent Time.	
3h 0m.	Neutral point below the sun distinctly seen
3 30	

1842, April 20.—Barom. 30·02. Very fine day; wind west.

Apparent Time.	Distance of Neutral Point from Sun.
12h 10m	11° 20′
12 37	10 40
2 21	12 0
3 45	12 35

The maximum polarisation was very great.

1842, April 25.—Thin hazy clouds before the sun.

Apparent Time.	
1h 11m.	Sun's alt. 45½°. Alt. neutral point below sun 34°. Distance below sun 11½°.

1842, April 26.—Barom. 30·08. Not a cloud in the sky from morning till night.

Apparent Time.	Distance of Neutral Point from Sun.
11h 1m	12° 15′
11 46	12 30
3 30	14 35
3 35	15 5
4 10	17 45

1842, April 27.—Barom. 30·04. Singularly fine sky.

Apparent Time.	Distance of Neutral Point from Sun.	
10h 45m	12° 5′	
12 12		A fog from the sea has just come on, and has driven the neutral point beneath the horizon, the bands being negative all the way to the horizon.
1 20		The fog has diminished. The neutral point is now seen near the horizon, playing up and down from 4° to 6° above the horizon, as the fog becomes denser or rarer!

1842, April 28.—Barom. 30. Fine sky; wind east.

Apparent Time.	Distance of Neutral Point from Sun.
11h 24m	10° 55′

1842, April 29.

Apparent Time.	Distance of Neutral Point from Sun.
11h 4m	11° 20′

1842, May 3.—China-ink sky; wind east.

Apparent Time.	Distance of Neutral Point from Sun.
11h 29m	10° 15′
12 15	9 25

1842, May 16.—Barom. 30·3. Thick haze; sun faintly seen.

Apparent Time.	
8h 49m A.M.	Neutral point below the horizon. The sun now quite hid, and no bands seen below him. A great glare of light, and a sort of *quaqua-versus* polarisation.

N.B.—When the neutral point is out of the plane passing through the sun, the zenith, and the observer, it arises in certain cases from there being a greater haze on one side of the plane than on the other.

1842, May 17.—Barom. 30·22. Haze all the day.

Apparent Time.	
$11^h\ 0^m$.	The neutral point beneath the horizon.
12 30	*Several neutral points, three at least below the sun, as the haze flies past in different thicknesses.*

1842, Aug. 17.—Barom. 28·8. Fine day; sky clear at 2^h.

Apparent Time.	
$2^h\ 9^m$.	Neutral point seen both below and above the sun.

1842, Aug. 28.—A diffused haze came over a bright blue sky.

Apparent Time.	
$3^h\ 49^m$.	The neutral point below the sun was almost in the horizon, and BABINET's neutral point near the zenith.
5 30	Sun invisible; mist thick; and the polarisation everywhere positive, and very feeble.

1843, Feb. 13.—Barom. 29·7. Fine sky.

Apparent Time.	Distance of Neutral Point from Sun.
$1^h\ 10^m$	15° 0′

1843, Feb. 16.—Barom. 29·15. Fine sky.

Apparent Time.	Distance of Neutral Point from Sun.
$12^h\ 57^m$	11° 25′

1843, April 30.—Barom. 30·07. Not a cloud. Neutral points distinctly seen, both above and below the sun.

Apparent Time.	
$4^h\ 15^m$.	Neutral point under the sun still above the horizon.

1843, June 15.—Barom. 30·03. Splendid day; wind east.

Apparent Time.	Distance of Neutral Point from Sun.
$12^h\ 13^m$	8° 10′
7 0	9 49

1844, May 3.—Barom. 30·15.

Apparent Time.	
$11^h\ 3^m$.	The neutral point below the sun distinctly seen. In order to see it well, I look at it perpendicularly through a plate of glass. The bands on each side of it are increased in intensity, the bands above being reversed.

When the sky is clear the neutral point under the sun approaches to the sun as his altitude increases, and coincides with the sun's centre when he is in the zenith.

As the neutral points of ARAGO and BABINET may be seen before the sun has risen, and after he has set, they are comparatively distinct and limited in their area; but as the neutral point below the sun never can be seen unless when the sun is shining, it has a less defined boundary and a wider area, owing to the flood of light in which it is generally enveloped. Hence arises the great difficulty of seeing it, and of detecting the form of the lines of equal polarisation which surround it. For the same reason, we can hardly expect to see the secondary neutral point, which must accompany it, when it rises or sets on a sea horizon in a condition to produce that phenomena.

On the Place of Maximum Polarisation, and its Intensity.

Next in importance to the determination of the place and movements of the three neutral points is the determination of the place and intensity of the maximum polarisation of the atmosphere.

In order to obtain these elements, a *polarimeter*, or instrument for measuring degrees of polarisation, is required. M. ARAGO constructed a very ingenious polarimeter, and I have described two forms of a polarimeter in the "Transactions of the Royal Irish Academy for 1841;"* but these instruments are too complex to be used from hour to hour during transient conditions of the atmosphere, when observations must be made with great facility and quickness. I was therefore obliged to use the following instruments.

1. Into one end of a tube 5 or 6 inches long and $1\frac{1}{4}$ of an inch wide I inserted a band polariscope, and in the axis of the tube I placed in a trough several (six to twelve) well annealed thin glass plates with their surfaces inclined to the axis of the tube at such an angle as to equal or compensate the average maximum polarisation to be measured. This compensation was effected more simply by adding or removing one or more plates when those in the trough had been previously placed at a fixed angle to the axis of the tube. It is obvious that, by giving the pile of plates a motion in one plane so as to vary the angle of refraction of the incident light, we should have an instrument for measuring all degrees of polarisation. I preferred, however, to use a polarimeter, all the parts of which were absolutely fixed.

In looking through this instrument we have a circular field SA, and when we direct it to the region of maximum polarisation, with the polarised bands parallel to SA, S being towards the sun, we shall see an interruption in the bands somewhere between S and A. If this interruption, or point of compensation, is at the point 2 in SA, I call S_2 the measure of the maximum degree of polarisation at the time of observation. After some practice, I had no difficulty in estimating by the eye when the neutral line was at 1 or $1\frac{1}{2}$, or 2 or $2\frac{1}{2}$, without placing marks at 1, 2, 3, &c., on a plate of glass at the end of the tube.

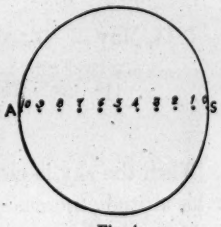


Fig. 4.

Having found that S_0 S_1 S_2, &c., corresponded with degrees of polarisation, measured by the rotation of the plane of polarisation, I thus had a measure of the maximum polarisation of the atmosphere at the time of observation.†

When it was necessary to measure very small degrees of polarisation, I preferred using a polarimeter with a single plate, to one with a pile of plates

* Vol. xix. part ii.

† See Phil. Trans. 1830, pp. 69, 133, 145, 287, and Trans. Irish Acad., vol. xix. part ii.

receiving the light at very small angles of incidence. This instrument, shown in the annexed figure, consists of two tubes of the same length, one of which, EFHG,

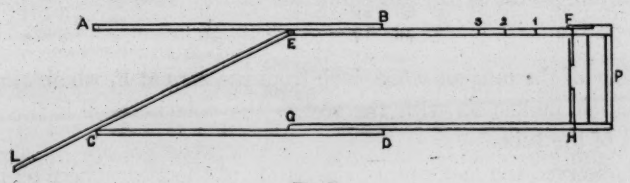


Fig. 5.

moves within the other ABDC. One plate of glass, EL, longer than AB, moves round a joint at E the end of the tube EFHG, resting upon C the end of the tube ABDC, so that when this tube EFGH is pushed in, the plate EL forms a greater angle with the axis of the tube, and when it is pulled out a smaller angle.

Now, if AB=EF, BF will be = AE, the tangent of the angle ACE, AC being radius, or of the angle of incidence of rays that fall upon EL parallel to the axis of the tube. The degrees of rotation, R, therefore, of the planes of polarisation produced by the refraction of the plate EL at different inclinations to the axis of the tube may be calculated from the formulæ—

$$R = \varphi - 45°, \text{ and}$$
$$\text{Cot. } \varphi = \cos^2.(i - i');$$

i being the angle of incidence, and i' the angle of refraction. The values of R, the measures of the degrees of polarisation, being laid down on the tube EF, we have a polarimeter which gives us direct measures of the polarisation of atmospherical, or any other kind of polarisation, when it does not exceed the maximum polarisation produced by the refraction of one plate. In this instrument the *zero* of the scale is at F, and B is the index.

In place of one plate, EL, we may use two, three, or more plates, and thus obtain a measure of all degrees of polarisation. If n is the number of plates, the value of R will be as follows:—

$$R = \varphi - 45°,$$
$$\text{Cot. } \varphi = \cos^n (i - i').$$

In the instrument which I constructed the radius AC was 1·13 inch, or $\frac{33·9}{30}$ inch, and the scale on FB was laid down from the following table, the index of refraction of the plate EL being 1·51.

Angles of Incidence i.	Angles of Refraction i'.	Rotation R.	Length of Tangent.
0° 0	0° 0′	0′	
30 0	19 20	1	0·655 inches.
40 34	25 31	2	0·968
47 46	29 22	3	1·244
53 17	32 4	4	1·515
57 39	34 1	5	1·784
61 25	35 3	6	2·074

Angles of Incidence i.	Angles of Refraction i'.	Rotation R.	Length of Tangent.
64° 40′	36° 46′	7	2·386 inches.
67 29	37 43	8	2·726
70 0	38 29	9	3·104
72 19	39 7	10	3·544

By setting off the tangent 0·655 inch from the zero at F, we obtain the place of 1° of rotation, and so on with the rest. The polariscope P is then placed at the end FH of the tube.

Having observed the maximum polarisation of the atmosphere by the polarimeter, and its place, we take its altitude A, and by means of the sun's altitude A′, observed or computed, we obtain the distance D of the place of maximum polarisation from the sun:—

$$D = 180° - \overline{A + A'}.$$

The following are all the values of D which I obtained. I have added the values of R or the degrees of polarisation in the zenith and in the horizon, when they happen to have been measured:—

Observations in 1841.

	Apparent Time.	Values of D.	Rotation in Zenith.	Rotation in Horizon.
April 28.	3h 0m	88° 16′	...	...
,, 30.	2 5	79 15	...	...
May 17.	1 20	99	18½°	15½°
June 6.	4 45	+90	16½	22½
,, 9.	4 30	+90	25½	24½
,, 10.	10 39	92 35	...	...
,, 10.	12 0	88 26	30 15′	29
Sept. 15.	10 18	88 4	27 0	26½
Oct. 26.	4 30	93	28¼	27½
Dec. 17.	9 7 A.M.	Appt. +90	27	24½
,, 18.	9 0 ,,	time. +90	28	27

Observations in 1842.

	Apparent Time.	Values of D.	Rotation in Zenith.	Rotation in Horizon.
Jan. 7.	9h 0m A.M.	120°	Haze	24½°
April 5.	6 58	+90	30½°	26½
,, 20.	12 49	84	25	17
,, ,,	2 25	86	25	19
,, ,,	3 35	88 20′	25	...
,, ,,	4 32	90	27	17
,, 21.	11 0	90	23	...
,, ,,	12 0	90	23	...
,, ,,	1 10	90	...	25
,, 25.	1 17	90	26½	...
,, 26.	10 53 A.M.	90	29	...
,, ,,	11 46	88	29	...
,, ,,	3 42	88	29	...
,, 27.	10 41 A.M.	87	29	...
,, 28.	11 34 A.M.	87	29	...
,, 28.	1 50 ,,	88	29	...
,, 29.	1 34 ,,	87½	29	...
May 3.	11 29 ,,	89	20	...

Omitting the three extreme values of D,—viz., 79° 15′, 99°, and 120°,—the mean of all the other values is 89°; but, considering that five of the values of D are marked as more than 90°, we may conclude that 90° is, in the normal state of the atmosphere, the distance from the sun of the place of maximum polarisation, and 45° the corresponding angle of incidence.

This determination of the place and angle of maximum polarisation affords a highly probable explanation of the *azure* colour of the sky. Sir ISAAC NEWTON considers this colour as a "*Blue* of the first order, though very faint and little, ... for all vapours, when they begin to condense and coalesce into small parcels, become first of that bigness whereby such an azure must be reflected."*

Professor CLAUSIUS considers the vapours to be vesicles or bladders, and ascribes the *blue* colour of the first order to reflection from the thin pellicle of water. In reference to these opinions the following facts are important:—

1. The azure colour of the sky, though resembling the *blue* of the first order, when the sky is viewed from the earth's surface, becomes, as observed by Mr GLAISHER in his balloon ascents, an "exceedingly deep *Prussian blue*" as we ascend to the height of five or six miles, which is a *blue of the second or third order*.
2. The maximum polarising angle of the atmosphere being 45°, is that of air, and not that of water, which is 53°.
3. At the greatest height to which Mr GLAISHER ascended, namely, at the height of five, six, and seven miles, where the blue is the brightest, "the air is almost deprived of moisture."

Hence it follows that the "exceedingly deep Prussian blue" cannot be produced by vesicles of water, but must be caused by reflection from the molecules of air, whose polarising angle is 45°. The faint blue which the sky exhibits at the earth's surface, is therefore not the blue of the first order, and is merely the blue of the second or third order rendered paler by the light reflected from the aqueous vapour in the lower regions of the atmosphere.

Immediately after the values of D, I have placed the values of R, or the degree of maximum polarisation, in order to show the relation between these two quantities; but as the values of D were taken only when it was convenient, the numbers R do not show the maximum intensity of the polarisation of the atmosphere. I have therefore selected the following from several hundreds of observations recorded in my journal.

	Mean Time.	Rotation in Zenith.	Rotation in Horizon.
1841, April 13.	$7^h\ 32^m$	$32\frac{1}{2}°$	...
,, ,, 16.	7 37	32	29°
1841, May 14.	4 30	$30\frac{1}{2}$	25
,, ,, ,,	7 10	$30\frac{1}{2}$	$2\,1\frac{1}{2}$

* Newton's Optics. 3d Edit. Book ii. part 3. Prop. vii. p. 232. See also Prop. v. p. 228, from which it would appear that by "small parcels" Newton meant solid globules of water.

	Mean Time.	Rotation in Zenith.	Rotation in Horizon.
1842, May 14.	$7^h\ 35^m$	$30\frac{1}{2}°$	29°
1842, Sept. 29.	4 37	$30\frac{1}{2}$	...
Very frequently the value of R was		29	...

The following observations show the changes which take place in the maximum polarisation in a few hours :—

	Mean Time.	Rotation in Zenith.	Rotation in Horizon.
1841, May 12.	$4^h\ 12^m$	$30\frac{1}{2}°$	25°
,, ,, ,,	6 0	27	$24\frac{1}{2}$
,, ,, ,,	7 10	$30\frac{1}{2}$	$28\frac{1}{2}$
,, ,, ,,	7 35	$30\frac{1}{2}$	29
,, ,, ,,	7 45	$28\frac{1}{2}$	$28\frac{1}{2}$
	Apparent Time.		
1842, April 16.	6 0	23	Much less
,, ,, ,,	6 33	24	$21\frac{1}{2}$
,, ,, ,,	6 59	$27\frac{1}{2}$	$20\frac{1}{2}$
,, ,, ,,	7 23	$29\frac{1}{2}$	$24\frac{1}{2}$
,, ,, ,,	7 37	32	$29\frac{1}{2}$
,, ,, 20.	4 44	$27\frac{1}{2}$	$17\frac{1}{2}$
,, ,, ,,	5 54	$28\frac{1}{2}$	...
,, ,, ,,	6 53	28	...
,, ,, ,,	7 1	$29\frac{1}{2}$	...
,, ,, ,,	7 24	30	...
,, Sept. 13.	5 58	$29\frac{1}{2}$	...
,, ,, ,,	5 31	29	...
,, ,, ,,	6 38	$29\frac{1}{2}$	...

	Apparent Time.	Rotation in Zenith.	Rotation in Horizon.
1842, Dec. 24.	12 3	$26\frac{1}{2}$	...
,, ,, ,,	12 44	28	...
,, ,, ,,	1 32	27	...
,, ,, ,,	2 24	29	...
,, ,, ,,	3 8	$27\frac{1}{2}$	...
,, ,, ,,	3 39	$29\frac{1}{2}$	...
,, ,, 27.	11 48	$26\frac{1}{2}$	...
,, ,, ,,	12 0	$23\frac{1}{2}$	$16\frac{1}{2}$
,, ,, ,,	1 45	24	...
,, ,, ,,	2 35	$27\frac{1}{2}$	...
,, ,, ,,	3 15	29	...
,, ,, 28.	11 38	29	...
,, ,, ,,	11 48	$27\frac{1}{2}$	$18\frac{1}{2}$
,, ,, ,,	11 58	27	$18\frac{1}{2}$
,, ,, ,,	2 31	$16\frac{1}{2}$!	Hazy
1843, Feb. 16.	3 28	$29\frac{1}{2}$	$24\frac{1}{2}$
,, ,, ,,	4 3	$29\frac{1}{2}$	$26\frac{1}{2}$
,, ,, ,,	5 4	30	...

The great rotation, amounting to $32\frac{1}{2}°$ on the 13th April 1842 at $7^h\ 32^m$, the greatest ever observed, was occasioned by an unusually favourable state of the sky. (See pp. 230 and 235.) I consider 30° as the maximum rotation in a normal state of the sky.

Having, in a normal state of the atmosphere, fixed the locality of the three neutral points, and determined the place and degree of maximum polarisation, we have the means of ascertaining approximately the form of the lines of equal polarisation, and of constructing a map of them when the sun is in the horizon.

In a paper published in Johnston's *Physical Atlas*, I have shown how this may be done, and have given two projections of these lines—one on a plane passing through the zenith of the observer, and perpendicular to the line joining the observer and the sun, and the other on the plane of the horizon. These two projections on a reduced scale, and without any of the numbers upon the larger ones, are given in Plate XII. and in the *Physical Atlas*. It will be seen from these projections that the lines of equal polarisation approximate to lemniscates, like the isochromatic lines in biaxal crystals.

On the Polarisation of Clouds and Exhalations.

The polarisation of clouds and other vapours presents some interesting phenomena, and should be studied in climates more genial than ours.

On the 29th June 1850, at about $8^h\ 30^m$ P.M., several reddish-white clouds appeared in the south-west sky at different heights, and in the zone of maximum polarisation. They were illuminated by the setting sun, and the sky around, and of course behind them, was of a deep blue. Upon looking at one of these clouds through Nicol's prism, I found that its reddish-white light was polarised in a plane at right angles to that in which the light of the sky was polarised. When the sky was dark by the disappearance of the blue polarised light in one position of the prism, the cloud was bright; but when the sky was brightest in a rectangular position of the prism, the cloud was of a dark blue colour.

July 15, 1850, $9^h\ 12^m$ P.M.—All the clouds to the east of the plane passing through the sun and moon, between the south-west and south, are black seen against the sky; but when we view them with a Nicol's prism, so as to extinguish as much as possible the polarised light of the sky, the clouds are white seen against the dark sky. When the Nicol's prism is turned round 90°, they again become black.

July 1, 1850, $8^h\ 30^m$ P.M.—A fine rainbow, with the secondary and supernumerary bows, appeared in the south-east. When the bands of the polariscope crossed either of the two bows at right angles, the bands at the intersection were very brilliant. When the rainbows were invisible from the great faintness of their light, they became visible, that is, the invisible portion became visible, when crossed with the bands of the polariscope. This effect did not seem to be produced when the bands crossed the supernumerary bows.

When the sun shines upon a light transparent vapour interposed between the observer and terrestrial objects, these objects are indistinctly seen through the light reflected by the vapour. As this light is partially polarised, it may be extinguished by a Nicol's prism, or a pile of thin plates of glass, or by reflection at the polarising angle from a glass plate. The terrestrial objects are then seen with great distinctness. This mode of obtaining improved vision of objects imperfectly visible, or of seeing objects not otherwise visible, may occasionally be of great use at sea.

On the Theory of Atmospherical Polarisation.

When the atmosphere is illuminated by the sun, his rays fall upon the aerial particles which compose it at all angles of incidence. In the immediate vicinity of the sun, where the angle of incidence is 180°, there is no polarisation. The polarisation increases with the angle of incidence, and becomes a maximum, as we have seen, at about 90° from the sun. It now diminishes with the angle of incidence, and becomes nothing at 180°, the point opposite to the sun.

At all these points the polarisation is said to be *vertical*, being in the vertical plane passing through the sun and the observer.

In addition to the vertical polarisation produced by the direct illumination of the aerial particles, there must be an opposite polarisation by which the neutral points are produced. M. Arago, M. Babinet, and, we believe, every other writer on the subject, have sought for this counter-polarisation "in the secondary illumination which the same aerial particles receive from the reflexion of the rest of the atmosphere, which sends to them light polarised horizontally,"* or oppositely to the light polarised vertically. That is, all the phenomena of atmospherical polarisation are produced by the opposite action of two lights polarised by reflexion, the one vertical, arising from the direct illumination of the aerial particles, and the other horizontal, produced by a secondary illumination of the same particles by the rest of the atmosphere.

This theory of atmospherical polarisation, omitting all consideration of the light polarised by refraction, never appeared to me satisfactory. There is no evidence whatever that such a secondary reflexion exists, even in a perfectly cloudless sky, and still less evidence that, if it did exist, it would be capable of neutralising the light polarised by reflexion at considerable distances from the antisolar point. It must be very feeble when the neutral point is about to disappear at the close of twilight; and as the polarisation by direct reflexion must be visible when the secondary reflection ceases to be visible, this cessation ought to be marked by a return of the neutral point to the antisolar point, the place which it would occupy were there no secondary reflexion.

Were the neutral points produced by a secondary reflexion, their distances from the antisolar point and from the sun ought to be affected when the sky is more or less covered with clouds; but though I have observed the neutral point of Arago in a clear part of the sky, I never observed that its distance from the antisolar point was changed when the rest of the atmosphere was obscured by clouds.

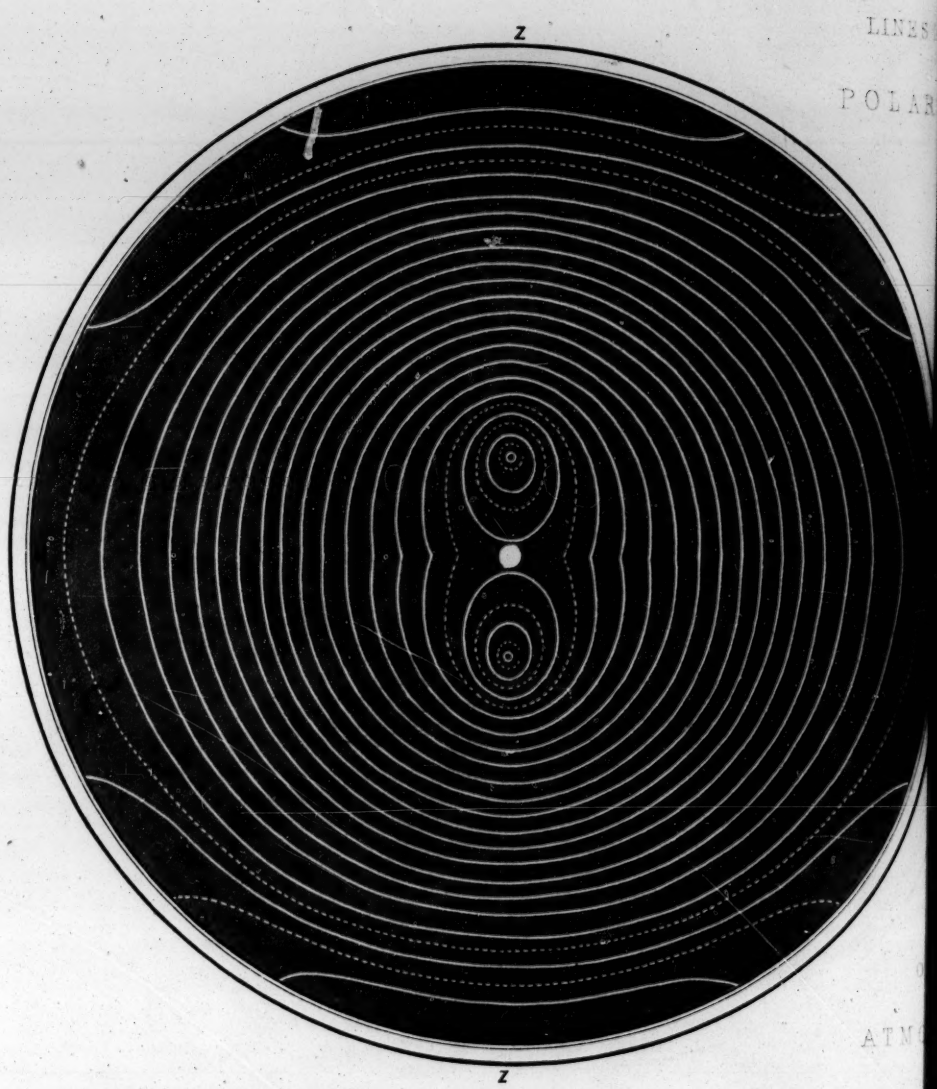
On these grounds I was led to the opinion that the neutral points must be produced by the opposite action of two polarised lights which had nearly the same relative intensity, and this opinion was strengthened by observations which I had made on the polarisation of light by refraction and transmission through piles of glass plates. In these experiments, published in the "Philosophical Transactions" for 1814, I observed phenomena analogous to neutral points, that is, the co-existence in the transmitted light of rays polarised by reflexion, and rays polarised by refraction, but I did not observe the effect where the intensities of these rays were such as to neutralise each other.

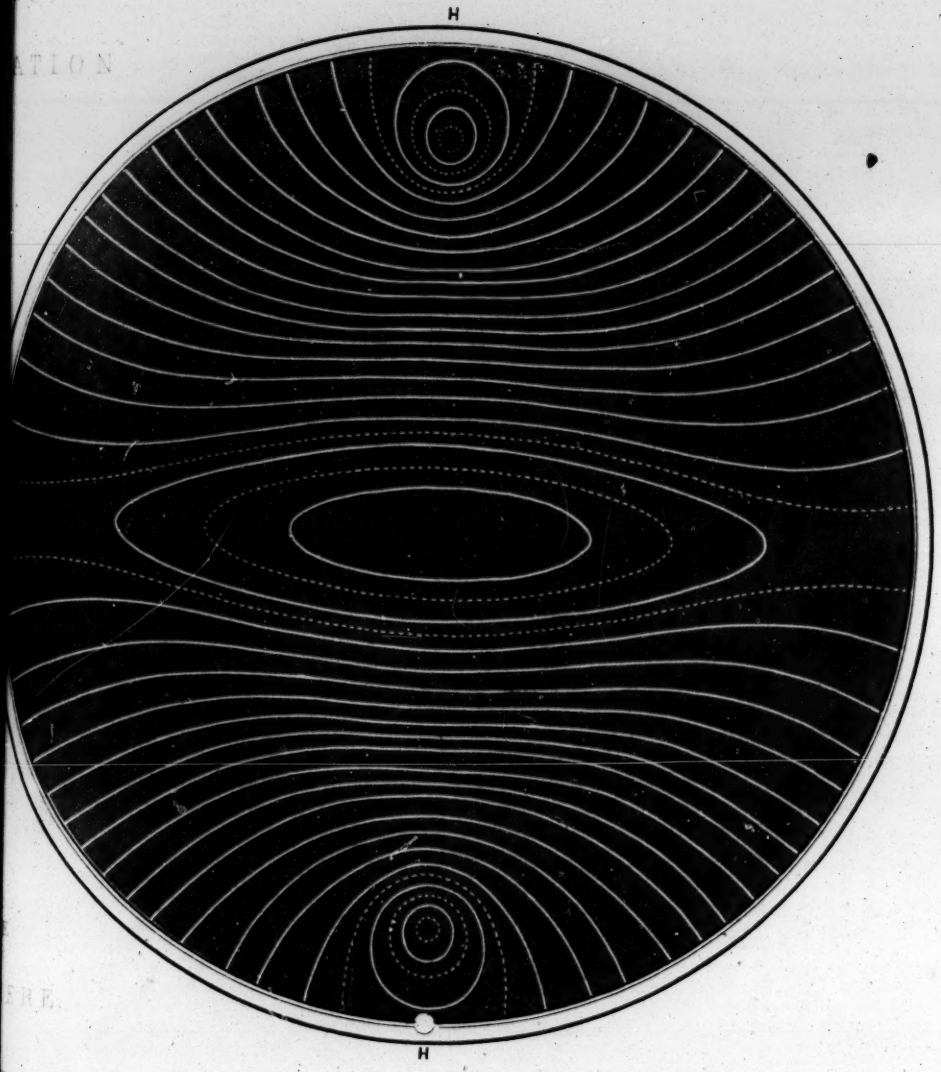
Guided by these views, I never doubted that the three neutral points in the atmosphere, and the partial polarisation of the light which it reflects, are produced by the opposite action of lights polarised by reflexion and refraction; and con-

* Babinet, Comptes Rendus, &c. 1846. Tom. xxiii. p. 233.

sidering the congeries of separate molecules which constitutes the atmosphere as a rough or dispersing surface, I made a series of experiments on such surfaces as were likely to act upon light in the same manner as the aerial molecules. In these experiments, which have been already communicated to the Society,* I found that such surfaces not only polarised partially the incident light, but produced neutral points, like those of the atmosphere, by the opposite action of the rays which they polarised by reflexion and refraction.

* See page 205.


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XXII.—*On a Pre-Brachial Stage in the Development of Comatula, and its importance in relation to certain Aberrant Forms of Extinct Crinoids.* By Professor ALLMAN. (Plate XIII.)

(Read 16th February 1863.)

While engaged last autumn in examining with a hand-lens the contents of a phial into which I had transferred some of the refuse of the dredging-boats employed in the oyster fishery on the coast of South Devon, my attention was attracted by a minute organism which adhered to a fragment of one of the larger Sertularidans. Under this low power it resembled somewhat a *Campanularia*, with the polype expanded; but, on being removed with a portion of the substance to which it was attached, and placed in a glass trough under the compound microscope, I found that it had closed up, and now resembled in form a cup surmounted by a pyramidal lid, and supported on the summit of a long jointed stem (Plate XIII., fig. 3). After it had remained for some time in this condition, I observed the pyramidal lid begin to open by the separation of its sides, and numerous long flexile appendages to issue from the cup (figs. 1 and 2).

I saw that I had now before me a remarkable crinoidal type, likely to be of much significance in its bearing on the general morphology of the order, and more especially interesting, as affording a key to the nature of certain extinct genera of Crinoids. It is the object of this communication to give some account of the points, which a very careful examination has enabled me to make out in the only specimen which I was fortunate enough to obtain.

For convenience of description, the little Crinoid may be divided into the body and the stem. The body consists of a calyx or cup-like portion, covered by a pyramidal roof. The calyx is composed chiefly of five large plates, very distinct, and united to one another by simple suture. Between the lower edge of these plates and the summit of the stem is a narrow zone, in which no distinct indications of a composition out of separate plates can be detected. Between the upper angles of every two contiguous large plates there may, with some care, be made out a minute intercalated plate; there would thus be five of these little intercalated plates, which, though by no means so evident as the five large plates which alternate with them, are sufficiently so to leave no doubt of their presence. The entire length of the body and stem is about $\frac{1}{12}$th of an inch, and that of the body alone about $\frac{1}{20}$th of an inch.

The pyramidal roof which closes the cup in the contracted state of the animal is composed of five large triangular plates, each supported by its base upon the upper edge of one of the large plates of the calyx, and with the small intercalated plates encroaching upon its basal angles. When the animal is

retracted, the triangular plates become approximated to one another at their edges, and then each will form the side of a pentangular pyramid. The height of this pyramid is about equal to the depth of the cup, so that the entire body has then somewhat the appearance of two nearly equal pyramids placed base to base; but when the animal is about to expand, the sides of the roof separate from one another, and opening outwards like the segments of a calyx in an expanding flower-bud, allow the flexile appendages already mentioned to come into view; and these, gradually elongating themselves, finally fall down gracefully over the edge of the calyx (fig. 1).

Both the calyx and roof are solidified by the deposit in them of mineral matter. The deposition, however, takes place in such a way as to leave a multitude of minute spaces free from it, so that the whole presents a kind of reticulated structure, which, while it is a characteristic echinodermal feature, adds much to the elegance of the little Crinoid. The membranous basis in which the earthy matter is deposited extends a little beyond the margin of the valve-like plates of the roof, and by transmitted light may be seen forming also a narrow transparent contour round the entire body.

Long flexile appendages, or cirri, have already been mentioned as rising out of the calyx. These, in the expanded state of the animal, are thrown out between the edges of the five diverging plates of the roof, and thence hanging down all round over the calyx, afford an additional element in the beauty of our little Crinoid. I have counted fourteen of these cirri, but they may be more numerous, for it is very difficult to determine their exact number. They appear to be cylindrical, with a canal occupying their axis; and as far as they can be traced backwards, they are seen to be furnished with two opposite rows of rigid setæ or fine blunt spines (fig. 4). Between every two opposite setæ a transverse line may be seen stretching across the cirrus, and indicating its division into tranverse segments. Of the base of the cirri I can say nothing. I have never succeeded in getting a view sufficiently deep into the space included within the five roof-plates, to allow of my tracing those appendages to their origin.

Besides the long extensile cirri now described, there is also an inner circle of short, apparently non-extensile, appendages. It was only occasionally that I succeeded in getting a glimpse of these; they appear to constitute a circle of slightly curved rods or narrow plates, probably five in number, which arch over the centre, and are provided along their length with two opposite rows of little tooth-like spines; they seem to be articulated to the upper or ventral side of the calyx by their base, and may be seen in a constant motion, which consists in a sudden inclination upon their base towards the centre, followed immediately by a resumption of their more erect attitude.

Our little Echinoderm is very irritable, and on the slightest annoyance the cirri are suddenly withdrawn, and the valve-like plates of the pyramidal roof

instantly closed down over them. In this retracted state it may remain for a long time, wearying the patience of the observer, who may have to wait for hours together before he can again obtain a satisfactory display of its structure.

The interior of the calyx is occupied by a reddish-brown visceral mass, obscurely visible through the walls; but I have not succeeded in obtaining a glimpse of the mouth, nor have I been able to discover the anal aperture.

The stem consists of a column, composed of a pile of segments solidified, like the walls of the body, by the deposit in them of earthy matter, and presenting also the same peculiar reticulate structure. Three or four of the upper segments are somewhat globular or bi-conical, giving a moniliform appearance to this part of the stem, while lower down the segments become cylindrical. Those situated near the middle of the stem have a prominent ridge running round their centre; while in those nearer the root the ridge disappears, and a simple line takes its place. Indications of a differentiated axis are visible through the whole length of the stem. The stem admits of slight flexure from side to side, and during life the animal may be every now and then seen swaying through a small arc. The whole of the stem is enveloped in a transparent membrane, which is particularly distinct on the upper part, and is a simple continuation of that which has been already mentioned as investing the body.

The multiplication of the segments in the stem seems to take place by the division of the pre-existing ones, and this division seems indicated by the transverse ridges which, in several of the segments, may be seen running round the centre.

In attempting to determine the exact affinities of the remarkable little organism now described, an important question presents itself at the outset: Are we to regard it as a mature form, or only as a transitional state in the development of some other? Everything tends towards the adoption of the latter view, and I think there can be no doubt that the little Echinoderm now described is one of the early stages in the development of a *Comatula*. That it had been seen by J. V. Thompson seems evident, from his celebrated memoir on "Pentacrinus Europæus,"* where some of the figures which accompany that memoir appear to be made from an organism identical with that here described, though the want of completeness in the description, and of sufficient detail in the figures, have left this part of the history of *Comatula* in a very imperfect state.

Again we find what we must regard as the same organism, figured by Dujardin. This naturalist figures in plate i. fig. 13 of his *Histoire Naturelle des Zoophytes Echinodermes*, a minute animal, which he observed at Toulon in 1835, and which he considers as the young *Comatula* at the commencement of its stationary existence. I have no doubt that the distinguished French naturalist had before

* John V. Thompson, "Memoir on the Pentacrinus europæus." Cork, 1827.

him, when he made his drawing, the little animal which forms the subject of the present paper, or at all events, the same phase of a nearly allied species; but his figure, while it gives a better idea of the form of the roof than THOMSON's, throws no light on the composition of the calyx, and is in other respects so deficient in detail as to render it of little use in working out the relations and significance of the little Crinoid.

Taking for granted, then, that the subject of the present paper is a pre-brachial stage in the development of *Comatula*, it will hold an intermediate position between the free larvæ described by BUSCH,* and the fixed *Pentacrinus* stage described by J. V. THOMPSON.†

A comparison of our larva with the adult *Comatula* is full of interest. In order, however, that this comparison may be made with advantage, it will be necessary, in the first place, to endeavour to assign their true value to the several plates which enter into the composition of the body-walls of the larva.

Now, the narrow zone which intervenes between the five large hexagonal plates and the summit of the stem, corresponds undoubtedly to the centro-dorsal piece which, in the adult *Comatula* carries a set of articulated appendages. Whether this piece is to be regarded as the homologue of the true basilar zone in the typical *Crinoidea*, or rather as a modified superior joint of the stem, is a point not immediately obvious. I entirely agree, however, with JOHANNES MÜLLER, in viewing the centro-dorsal piece in *Comatula* as a metamorphosed stem joint; for not only do we fail to detect in this portion of the calyx a composition out of distinct plates, but, what is of more importance, it is from this very piece that the articulated appendages are developed,—these being, as shown by the analogy of *Pentacrinus*, properly appendages of the stem.

If this be the true interpretation of the part in question, then the five large plates, which are superimposed upon it in our larva, and which here constitute almost the whole of the calyx, will correspond to the true *basalia*, which immediately surmount the stem in such forms as *Platycrinus*, and the great majority of the *Crinoidea*, while the five small plates intercalated between their upper angles will represent *radialia;* for it is manifest, as shown by the larval *Comatula* in a more advanced stage—for a preparation of which I am indebted to the kindness of Dr CARPENTER—that it is in the position of these little plates that the proper arms will subsequently arise.

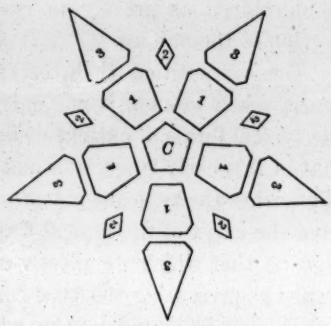
* BUSCH, "Beobachtungen über Anat. und Entwickel. einiger Wirbelosen Seethiere." Berlin, 1851.

† THOMPSON, *loc. cit.* Since reading the present paper, I met with an abstract of a paper on the "Embryogeny of *Comatula rosacea*," by Professor WYVILLE THOMSON, in the "Proceedings of the Royal Society of London" for Feb. 5, 1863. In this paper Professor THOMSON gives us the results of a series of valuable observations in which he has traced the development of the larva beyond the stage at which it was left by BUSCH, and has further elucidated its earliest stages. He has not, however, carried it up to the point which it is the object of the present paper to describe; while the very careful and complete researches of CARPENTER, referred to below, would seem to start from a point a little in advance of that here described.

I believe this to be the correct view of the parts in question; but if we should prefer to regard the single centro-dorsal piece as representing a zone of coalesced basalia, then we must view the five large plates as *parabasalia;* for that they are not radialia is manifest from the undoubted relations of the five small intercalated plates, which must still be considered as radialia, whatever interpretation we give to the centro-dorsal piece.

The signification of the five large triangular roof-plates still remains to be determined. Now, I have no hesitation in regarding these as *inter-radialia* greatly developed, and, in consequence of the slight development of the radialia, brought into contact with the upper edges of the basalia, to which they are united, not by immoveable suture, but by a moveable articulation. In accordance then with these views, the analysis of the body of *Comatula* in the pre-brachial stage, which constitutes the subject of the present paper, will stand as in the annexed woodcut, fig. 1.*

Fig. 1.



Plan of the Pre-Brachial Comatula.

C. Centro-dorsal Piece.
1. Basalia, ... 5
2. Radialia, ... 5
3. Inter-radialia, . . 5
Arms, ... 0

There is scarcely any more striking feature in our young *Comatula*, than the long tentacula-like appendages or cirri which, in the expanded state of the animal, are protruded between the edges of the five roof-plates. A few of those appendages, in an imperfectly extended state, are shown in DUJARDIN'S and THOMPSON'S figures above referred to. Though I have never succeeded in getting a view of their bases, I would regard them as ambulacral feet greatly developed, and otherwise peculiarly modified. Their general resemblance in structure to the "tentacula," which lie in the ambulacral grooves upon the arms, pinnulæ, and disc of the more advanced *Comatula*, is quite in accordance with this view.

It is more difficult to give a satisfactory account of the long inflexible toothed plates, which, in our larva, lie between the tentacula and the centre; and I confess myself unable to decide as to their real nature, for their structure seems inconsistent with the idea of their being an internal series of modified cirri.

We are now prepared to institute a comparison between our pre-brachial *Comatula* and the adult animal. This comparison will show a composition altogether different in the two cases. In the adult *Comatula*, the only distinct pieces

* Dr CARPENTER informs me that he has detected in an early stage of the larval *Comatula*, an unsymmetrical "anal" plate, which he regards as one of a first series of inter-radials (the rest of this series being usually abortive), and which is afterwards lifted by the development of the anal funnel into a much higher position. I have sought in vain for this plate in my specimen, and am of opinion that in the stage to which this specimen belongs, the plate does not exist. It is quite possible, however, that though it has here escaped detection, it may yet be found in specimens better suited for observation.

which we find in the calyx are a large centro-dorsal portion, carrying the articulated dorsal appendages, and the five radii, each composed of three radialia, the first of which is directly attached to the centro-dorsal piece, while the third carries two largely developed pinnulose arms. There is no trace of basalia, and no trace of the five triangular roof-plates of the larva. It will be thus seen, that instead of the five minute plates representing the radii in the larva, we have here the radii, consisting each of three well-developed radialia, and constituting the principal portion of the calyx, while the arms totally absent in the larva, are in the adult so developed as greatly to exceed in magnitude the whole of the remaining portion of the animal.

The composition of the calyx in our pre-brachial *Comatula* has a special interest, when we view it not only in relation to that of the adult, but to that of the typical forms of extinct *Crinoidea*. Its large and very distinct basalia show, that in this early stage it is not alone in the presence of a stem that *Comatula* approaches the typical *Crinoidea*, but that its calyx also, so different in the adult from the calyx of the typical forms, is here constructed upon essentially the same plan as that which we meet with in such genera as *Platycrinus*, which we may regard as presenting the type composition of this part.* As no trace of distinct basalia can be found in the adult *Comatula*, these elements must have either coalesced with some of the others in the progress of development, or, what is more probable, have been obliterated by the encroachment of the radii and centro-dorsal piece.†

In the fossil genus *Solanocrinus*, and in the living genus *Comaster*, founded by AGASSIZ for the *Comatula multiradiata* of GOLDFUSS, though afterwards suppressed by JOHANNES MÜLLER, as founded on characters which he had not been able to confirm in any living species, we have a *Comatula* in which five small basalia alternate with the radii, but are not large enough to form a continuous zone, separating the radialia from the centro-dorsal piece. *Solanocrinus* would thus represent a stage in the development of *Comatula* before the basalia had become entirely obliterated, and therefore intermediate between the subject of the present paper and the adult.

A series of highly valuable, but as yet unpublished observations, have been made by Dr CARPENTER, on the development of *Comatula*, in which he has traced the progress of the animal from a very early period after the first appearance of the arms to its final detachment from the stem. In the earliest of these stages, Dr CARPENTER has found the five roof-plates still present, and he informs me that

* It should be borne in mind, that while the basal zone of Platycrinus *appears* to depart from the type, by having only three distinct basalia, these pieces easily admit of a further analysis into the typical number five.

† From a letter which I have received from Dr CARPENTER since the above paragraph was printed, I find that he has instituted a nearly similar comparison between the calyx of the young Comatula and that of the adult typical crinoid.

he has seen them in the progress of development finally disappear by absorption. Dr CARPENTER's observations, which set out from a stage a little in advance of that of our larva, will doubtless throw much light on the steps which intervene between the stage here described and the fully-developed *Comatula;* they are illustrated by a beautiful series of drawings and preparations, and it is greatly to be desired that their author may not delay long in giving them to the public.

One of the most interesting features in our little larva, is the light which it seems capable of throwing upon the nature of certain extinct forms. The description by GUETTARD,* in the middle of the last century, of a living specimen of *Pentacrinus caput medusæ*, and the discovery by THOMPSON,† towards the beginning of the second quarter of the present century, that the *Comatulæ*, in their young state, are stalked Crinoids like *Pentacrinus*, must be regarded as the two grand steps in our knowledge of the morphology of the *Crinoidea* by which we have been enabled to establish the relations which exist between the extinct stalked forms and the dwellers in our present seas.

It is however the normal types of the fossil Crinoids that our knowledge of the living forms has hitherto chiefly tended to elucidate; and there are still numerous aberrant types, such as *Haplocrinus*, *Coccocrinus*, *Stephanocrinus*, &c., whose affinities can scarcely yet be regarded as in all respects satisfactorily determined. I believe that the little animal which forms the subject of the present paper will help us towards a clearer conception of the relations of some of these, and enable us to assign their true significance to certain points in their structure which have presented difficulties in the way of a satisfactory morphological analysis.

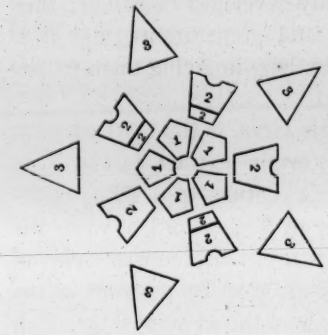
The Devonian genus *Haplocrinus* will here at once suggest itself—so remarkable by its tall pyramidal roof and rudimentary arms. In this genus, the summit segment of the stem undergoes no metamorphosis, and the centro-dorsal piece of *Comatula* is accordingly wanting while the stem is immediately surmounted by the basilar zone, consisting of five basalia. To this succeed the five radii, rendered irregular by two of these radii consisting each of a single radiale, while the three others are each composed of two radialia. Lastly, alternating with the radii, are the five sloping sides of the pyramidal roof, and these I think must be recognised as the exact equivalent of the five valve-like plates of the pre-brachial *Comatula*, and therefore *inter-radialia*, here separated from the basalia by the greater development of the radialia; for not only their position, but the discovery by JOHANNES MÜLLER of rudimental arms in this genus, is inconsistent with RÖMER's view, that the roof-plates are the arms of *Haplocrinus* in a condition similar to that of the

* Mém. de l'Acad. Roy. des Sciences, 1755.

† Memoir on the *Pentacrinus europæus*, Cork, 1827; and afterwards in the "Edinburgh New Philosophical Journal," 1836.

arms in *Cupressocrinus*. The following diagram, fig. 2, exhibiting an analysis of *Haplocrinus*, will render the relations here insisted on more apparent.

Fig. 2.



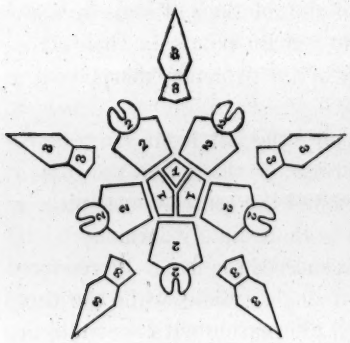
Plan of Haplocrinus.

1. Basalia, . . . 5
2. Radialia, . . . 3×2+2×1
3. Inter-radialia, . 5

Again, in the singular Devonian crinoid *Coccocrinus*, J. MÜLLER, we have the roof in the form of a depressed pyramid, composed chiefly of five large pointed pieces, whose apices meet in the centre, and which are also plainly *inter-radialia* and homologous with the sides of the pyramid which forms the roof in *Haplocrinus*, and in our larval *Comatula*, while five smaller pieces, constituting a first series of inter-radialia, are associated with them, as may be seen in the annexed diagram, fig. 3.

In the genus *Stephanocrinus* (woodcut, fig. 4), if we adopt the analysis given by J. HALL,* who has had abundant opportunities of studying this crinoid in the Silurian strata of the United States, the inter-radialia will be represented by the small triangular plates whose apical angles meet in the central point of the roof, and by the large plates which meet the bases of these, and have their sides prolonged vertically as five strong pyramidal spines which surround the margin of the roof, and give its characteristic crown-like form to this elegant little fossil; while the calyx will consist of a zone of three basalia, followed by a zone of five large radialia, which support the five small semicircular plates occupying the margin of the roof, and constituting a zone of distal radialia. The roof itself is completed by five pairs of long narrow parallel plates which alternate with the inter-radialia, and which must be regarded as axillary† plates. If, however, with RÖMER we view the spine-like plates, not as distinct elements, but as formed by the prolonged angles of two contiguous radialia, then the inter-radialia will be represented by the five small triangular apical plates alone.‡

Fig. 3.



Plan of Coccocrinus.

1. Basalia, ... 3
2. Radialia, ... 5 × 2
3. Inter-radialia, . . 5 × 2

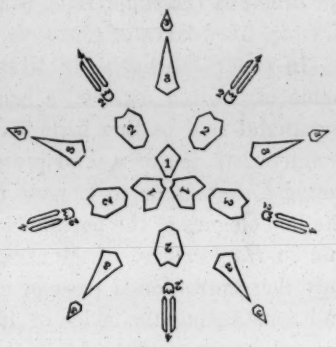
* Palæontology of New York.

† The term "axillary" is here used in the sense in which it is employed by DE KONINCK.

‡ I cannot, with RÖMER, PICTET, and BRONN, regard *Stephanocrinus* as a cystidian, a view opposed not only by the numerical law of its parts, but by the structure of its roof, and the position of its arms as determined by HALL, from which we must infer a considerable development for the

From *Stephanocrinus*, the transition is not difficult to the remarkable genus *Eucalyptocrinus*, in which the roof is greatly elevated, and is composed of five series of inter-radial plates alternating with five series of axillary plates, having the arms lodged in deep grooves between them (woodcut, fig. 5). If the arms and axillary plates were here suppressed, and each of the inter-radial series simplified by reduction in the number of its constituent plates, we should have the portion which surmounts the calyx, and gives so singular a physiognomy to this curious Crinoid, presenting essentially the same composition as in our larval *Comatula*.

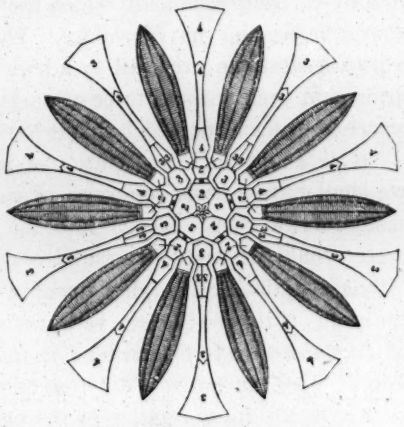
Fig. 4.



Plan of Stephanocrinus.
1. Basalia, ... 3
2. Radialia, ... 5
3. Inter-radialia, . . 5 × 2
4. Axillary, ... 5 × 2

In *Eugeniacrinus*, however, if we adopt the very elegant synthesis of this genus as proposed by QUENSTEDT,* we must, instead of regarding each of the five pieces which form the pyramidal roof as an inter-radial, interpret it as an axillary piece inseparably united to the last radial. The roof of *Eugeniacrinus* would thus have a different signification from that of the roof in the larval *Comatula*.

Fig. 5.



Plan of Eucalyptocrinus *after* DE KONINCK.
1. Basalia, ... 5
2. Radialia, ... 5 × 3
3. Inter-radialia, . . . 5 × 5
4. Axillary, ... 5 × 3

If in *Eucalyptocrinus* the inter-radial series were suppressed, while the axillary series, simplified by the reduction in number of their constituent elements, were, as the others became suppressed, correspondingly developed, so as to bring their lateral edges into contact; and, finally, if the arms were liberated from their adhesion to the roof,—this genus would, so far as the characters involved in the changes here supposed are concerned, be converted into a *Eugeniacrinus*. While, on the other hand, as we have already seen, the lateral development of the inter-radial series, and the entire suppression of not only the axillary series, but the arms, would so far convert it into the larval *Comatula*. *Eucalyptocrinus* thus

ambulacral zone, a character incon sistent with cystidian morphology. DUJARDIN with more reason places *Stephanocrinus* in his family of *Haplocrinidæ*. The so-called ovarian pyramid of this genus seems to me much more probably an anal orifice.

* Ueber *Eugeniacrinites caryophyllatus* in Neues Yahrbuch für Mineralogie, &c. 1855. P. 669.

occupies a middle place between *Eugeniacrinus* and *Haplocrinus*, passing on the one side, by the suppression of the inter-radial series, into *Eugeniacrinus*, and on the other, by the suppression of the axillary series, into *Haplocrinus* and the pre-brachial fixed larva of *Comatula*.

In the "Jahrbuch für Mineralogie" 1840, HAGENOW describes, under the name of *Hertha mystica*, a small Crinoid from the White Chalk. The high pyramidal roof of this little fossil, as figured by HAGENOW, is one of its most characteristic features, and presents so striking a similarity to the roof of our young *Comatula*, as to suggest the probability that its five triangular sides are the homologues of the parts which so closely resemble them in the latter animal and in *Haplocrinus*. J. MÜLLER, however, considers that the *Hertha mystica* is only the centro-dorsal piece of a *Comatula*, with the first radialia still attached and constituting the sides of its roof, a view which seems borne out by the general structure of the fossil, though its remarkable form is certainly something very different from anything presented by the same parts in the *Comatulæ* of the European seas. The discovery of additional specimens will probably clear up some of the difficulties in the way of an entirely satisfactory solution of the nature of this somewhat enigmatical little fossil.

DE KONINCK and LE HON (Recherches sur les Crinoides du Terrain Carbonifere de la Belgique) describe, under the name of *Lageniocrinus seminulum* de KON., a minute carboniferous fossil which they regard as a transitional form between the *Platycrinidæ* and the *Blastoidea*. The body of this little fossil is in the form of a pentahedral cup, crowned by a high pyramidal five-sided lid, quite destitute of arms, and even without trace of surfaces which could have served as points of attachment for arms which might have at one time existed.

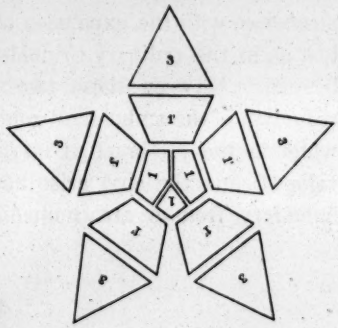
If we adopt the interpretation of its composition as proposed by DE KONINCK, we shall have a basis composed of three basalia (admitting, as in other similar cases, of an easy analysis into the typical number five); while with these there would alternate five radial series, each consisting of two radialia, all the ultimate radialia converging towards the summit, in order to form the sides of the roof. If this be the true view, then, notwithstanding the striking resemblance of *Lageniocrinus* to our larval *Comatula*, we must regard the roof of the latter as formed upon an entirely different plan from that of *Lageniocrinus*. Adopting DE KONINCK'S interpretation of the plates which enter into the formation of the calyx, we may compare the five roof-plates to those which form the vertex in *Eugeniacrinus* as described above, and therefore as properly axillary; or the flattened arms forming the roof of *Cupressocrinus* may also suggest themselves as the true homologues of the roof-plates in *Lageniocrinus*. But it is also quite open to us to regard the roof of *Lageniocrinus* as homologous with that of *Haplocrinus* or of our larva, only that in this case we must view the plates to which DE KONINCK assigns the place of first radialia as *parabasalia*, and we shall then

have the sides of the roof taking the place of *inter-radialia*,—in other words, having the exact value of the parts which so closely resemble them in the larval *Comatula*, while the radialia are either not developed at all, or are so minute as to have escaped notice in the fossil (woodcut, fig. 6). According to this interpretation the formula of the composition of *Lageniocrinus* would stand as follows:—

Basalia,	3	(two large and one small.)
Parabasalia,	5	
Radialia,	0	(or inconspicuous.)
Inter-radialia,	5	
Arms,	0	

Until we know more of the structure of this very rare little fossil, we shall be at liberty to adopt either of the above interpretations; but it may be remarked that the view taken of the composition of the calyx of *Lageniocrinus* in the second of these interpretations is exactly that which we must take of the calyx of the larval *Comatula*, viewed apart from the roof, if, with certain authors, we consider the centro-dorsal piece in *Comatula* as representing a zone of united basalia (see page 245); for then the basalia, in my analysis of the larva given above, will become *parabasalia*, a view, however, which, from reasons already stated, I am not disposed to adopt, and which is not necessary for the comparison here insisted on.

Fig. 6.



Plan of Lageniocrinus.

1. Basalia. 3
1′. Parabasalia, ... 5
3. Inter-radialia, ... 5

Between our pre-brachial fixed *Comatula* and the *Blastoidea*, with their high pyramidal five-sided roof, there is, on a superficial view, so strong a resemblance, that we are at first tempted to consider them as both constructed on the same plan. They have, however, really very little in common; for although the inter-radialia take part, sometimes largely (*Elæacrinus*) in the formation of the roof, yet the so-called *pseud-ambulacra*, with their singular and complicated structure,* and the system of ovarian apertures, seem to place the *Blastoidea* in a group so widely distant from *Comatula* as to afford but few points of real agreement with our larva.

With *Cupressocrinus* also, so very remarkable by its tall pyramidal shape, the resemblance is only apparent, the sides of the roof-pyramid in *Cupressocrinus* belonging without doubt to the radial, and not to the inter-radial system of the Crinoid.

* See Römer, Monographie der Blastoideen. Archiv. f. Naturg. 1850.

I do not think that the little animal which forms the subject of the present paper will throw much light on the nature of the *Cystidea*, though in some respects it reminds us of them. It is true that its long tentacula-like appendages forcibly recall by their position the arms, more or less strong indications of which have been shown by the observations of Volborth, Ed. Forbes, and J. Müller, to exist in probably all *Cystidea*. The long cylindrical appendages described by Volborth in *Echino-encrinus angulosus*, and *E. striatus*, and by Forbes in *Prunocystites*, will more especially occur to us in this comparison. No homological identity, however, can be admitted between the two sets of organs; for while the arms of the *Cystidea* are referable to the same category as those of the ordinary crinoids, and have a well-developed calcareous skeleton, the cirri of the young *Comatula* belong to the ambulacral system. The chief point of resemblance lies in an apparent identity of position; but it will be remembered that an essential character in the *Cystidea* is the reduction of the ambulacral zone to a *minimum*, with the expansion of the antambulacral zone to a *maximum*. Now, just as in the ordinary Crinoids, the arms of the *Cystidea* are situated upon the boundary between these two zones, and their origins are therefore, by the reduction of the ambulacral zone, necessarily carried inwards towards the vertex; while in the pre-brachial larva of *Comatula*, the ambulacral zone is well developed, and the cirri arise not from its peripheral boundary, but within this boundary, from an area immediately surrounding the mouth.

Description of Plate XIII.

Pre-Brachial Comatula greatly enlarged.

Fig. 1. The animal with its roof-plates fully expanded, and the cirri extended from between their edges.

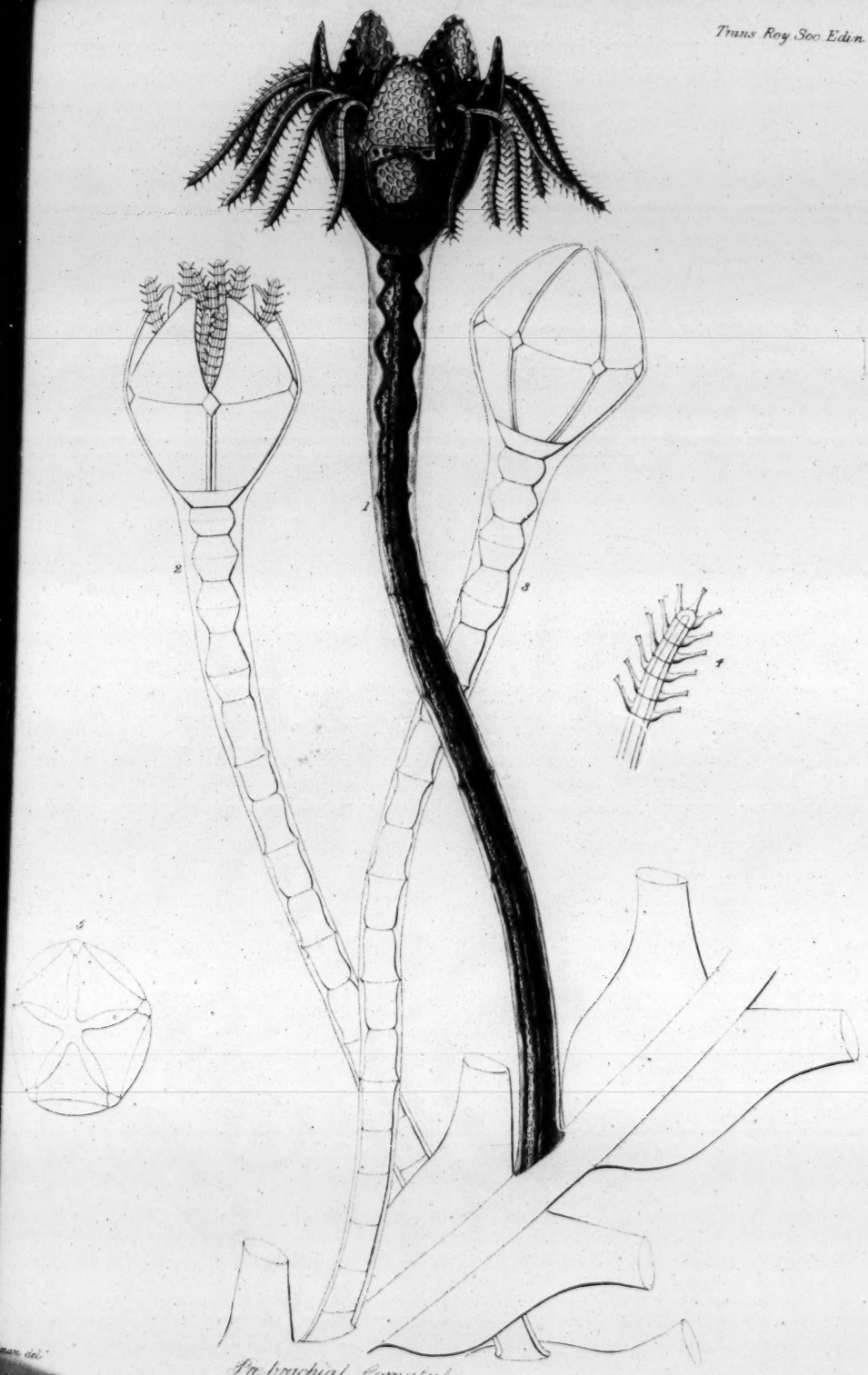
2. Outline of the same animal, in the act of expansion.

3. Outline of the same with the cirri entirely withdrawn, and the roof-plates closed.

4. The extremity of a cirrus still more magnified.

5. Outline of the body looking down upon it from the vertex.

Trans. Roy. Soc. Edin. Vol. XXIII. pl. 13



Pre brachial Comatula.

G. Jarman. ad

XXIII.—*Some Account of the Recent Progress of Sanskrit Studies.* By J. Muir, Esq., D.C.L., LL.D.

(Read 16th February 1863.)

In compliance with the desire which the Council have done me the honour to express, I have drawn up the following account of the recent progress and present state of Sanskrit studies, prefixing such an outline of the earlier history of these researches as may serve to complete the review, and render it more easily intelligible.

In this sketch I do not profess to communicate anything new, but merely seek to present such a summary of the results already obtained, as may convey to those who have not bestowed any special attention on the subject some idea of the character and affinities of the Sanskrit language, and of the nature and contents of Indian literature, as well as of the advances which have of late years been made in the principal branches of the study.

The ancient Greeks and Romans possessed a considerable amount of acquaintance with the institutions and opinions of the Hindus. The writer from whom their knowledge was principally derived was Megasthenes. In the course of his journey through northern India, and his residence as the ambassador of Seleucus Nicator at the Court of King Sandracottus, in the beginning of the third century B.C., this author made it his business to collect such information as he could obtain in regard to the country and its inhabitants. Though we have to regret the loss of the original work in which he embodied the results of his researches, yet in the extracts from it which have been preserved by other writers* we find a description of the physical features and productions of India; of the manner and customs of the people; of the administration of government; of the system of castes; of the learned men, whom he states to have been divided into two classes, the Brachmanæ and the Sarmanæ; of the philosophical discussions and lectures of the former, their doctrine that death is but the beginning of life, that nothing earthly is either good or bad,† that the world is created

* See Schwanbeck's Megasthenis Indica. Bonn, 1846.

† The religious character of the Indian philosophy is referred to in a story told by Aristoxenus the musician, as reported by Aristocles the Peripatetic (Eusebius, præp. Evang., xi. 3, 8). The story (for the truth of which, however, Aristoxenus does not vouch) is to the effect, that an Indian who had met Socrates at Athens, and had been told by him, in answer to a question he proposed, that the proper object of philosophy was human life, rejoined, with a scornful laugh, that no one could understand human things who was ignorant of things divine.

and perishable, that it is pervaded by the Deity, that water is the first principle of all things, that the material universe is composed of five elements, that the soul is immortal, &c.; while the Sarmanæ are represented as ascetics, living in the woods, and subsisting on the natural products of the forest.

The accounts which MEGASTHENES gives of the Indian deities are not very exact, as they are coloured by the tendency which he had in common with the rest of his countrymen, to discover in the gods of the barbarians the counterparts of the Greek divinities. It is supposed by scholars, that it is the Siva of the Indians whom he seeks to identify with Dionysus, and Krishna whom he describes as the Hercules worshipped by the Surasenæ at Methora.* MEGASTHENES also speaks of the long mythological legends of the Indians, and of the odes they sang in honour of their dead; but he denies that they were acquainted with writing.† Though many Sanskrit proper names are given (more or less disfigured) by the same writer, and though ARRIAN mentions the Sanskrit word for a palm-tree (*tāla*), it does not appear, as far as I am aware, from any of the accounts that have come down to us, that the Greeks knew anything of the Indian sacred language. We are not to suppose that they had any opportunities of studying the Indian books. All the information they have transmitted to us must have been derived from oral communications, made, most probably, through interpreters; for Greeks like MEGASTHENES were, we must presume, but little acquainted with even the Indian vernacular dialects. Even if Sanskrit was not unknown by name to MEGASTHENES, we cannot suppose that, with his limited opportunities of information, and in the state of linguistic science which prevailed in his time, he was competent to appreciate its character and importance.

As regards the early knowledge of Sanskrit in other foreign countries, it appears that when the Buddhist religion first spread from India into China, before the commencement of the Christian era, a knowledge of the sacred language of the Indians was introduced along with it. Sanskrit is stated to have become known to the Mohammedans also in the eighth century A.D.; and various works, medical, mathematical, and philosophical, were translated from that language into Arabic, at the period when learning flourished at the Court of the Khalifs.‡

Though the Mussulman rulers of India have for the most part exhibited little curiosity, and testified even less respect, for the literature of the Hindus, the case was different in the reign of the tolerant Emperor AKBAR, who, himself an eclectic in religious matters, encouraged the study of Sanskrit books, from which numerous

* A writer quoted by STRABO (xv. 1. 69, p. 718), and supposed by LASSEN (Indische Alterthumsk. ii. 697), to be also MEGASTHENES, says that the Indians worshipped Jupiter Pluvius (by whom Indra must be meant), the river Ganges, and the local deities.

† SCHWANBECK, pp. 89, 113.

‡ See Professor MÜLLER's Lectures on the Science of Language, p. 139.

translations were made into Persian by a liberal-minded Mohammedan named FEIZEE.*

It is further clear, that Sanskrit was well known to the Jesuit missionaries in the south of India in the earlier part of the seventeenth century. Prompt to perceive the use which might be made of the sacred language and literature of the Indians for the purpose of recommending to the Brahmans the doctrines of Christianity, these learned and zealous Roman priests composed in Sanskrit verse a number of theological treatises, which they entitled Vedas, and in which, under the names of celebrated Hindu sages, they refuted a variety of Indian errors, and paved the way for the direct inculcation of Christian doctrines. On account of the interest attaching to this subject, I shall give here a few particulars regarding these curious compositions. One of these works, translated into French, was published at Iverdon in 1778, under the title of "L'Ezour vedam, ou ancien Commentaire du Vedam, contenant l'exposition des opinions religieuses et philosophiques des Indiens, traduit de Sanscretan par un Brame." This book was supposed by the editor, the Baron ST CROIX, to be a genuine Indian work; and he says in his preface that the translation had formed part of the papers of M. BARTHELEMY, one of the members of Council at Pondicherry, and was presented to the Bibliotheque du Roi by VOLTAIRE, who also believed in its authenticity. It was known, however, to the French writer SONNERAT, and acknowledged by the Roman missionary FRA BARTOLOMEO in his "Systema Brahmanicum," that this work is in reality a controversial treatise composed by a Jesuit. This fact is in reality sufficiently established by internal evidence, but it has been placed beyond doubt by the proofs furnished by Mr FRANCIS ELLIS, in his dissertation on the subject in the fourteenth volume of the "Asiatic Researches."† This gentleman had ascertained by chance that the original of this work existed among the MSS. in possession of the Catholic missionaries at Pondicherry, which were understood to have at first belonged to the Jesuits. Besides this so-called "Ezour vedam" (*i. e.* Yajur-veda), there were also found among the MSS. in question imitations of the other three Vedas, each in Sanskrit in the Roman character, and in French; these languages being written on the opposite pages of the MSS., to give them the appearance of originals with translations annexed. Mr ELLIS gives an extract from the commencement of the "Sama Veda," in both languages, and states the substance of the five books of which that work is composed, from the abstracts in the MS. These abstracts are inserted in the margin of the French part, and are evidently intended for the information of the European reader only, the author's views being more explicitly declared there than in the text or translation. Thus it is

* ELPHINSTONE's History of India, 1st ed. vol. ii. p. 316.

† See also the Preface to the first volume of the "Christa Sangita, or Sacred History of our Lord Jesus Christ, in Sanskrit verse," by the Rev. W. H. MILL, D.D., late Principal of Bishop's College, Calcutta, pp. iv–ix.

said that the second chapter of the first book "refute la fausse idée que les faux Veds donnent de la Divinité." The "Faux Veds" are also spoken of again. In page 22, Mr ELLIS remarks, that the Sanskrit portion of the particular MS. to which he is there referring contains many alterations and variations of reading in the same hand, either inserted in the margin or interlined. These various readings sometimes correct, sometimes alter the sense, and are such as an author only would make in an original work. In page 30 Mr ELLIS writes thus, in regard to the author of these works: "There prevails among the more respectable native Christians of Pondicherry an opinion, on what authority founded I know not, that these books were written by ROBERTUS DE NOBILIBUS.* This personage, of the Society of Jesus, and the founder of the Madura Mission, long the most flourishing of any that ever existed in India, is well known, both by Hindus and Christians, under the Sanskrit title of TATWA-BODHA-SWÂMI, as the author of many excellent works in Tamil on polemical theology." Both of his works, entitled "Atma-nirnaya-vivecam," and "Punarjanma âkshepa," "in style and substance greatly resemble the controversial part of the pseudo-Vedas; but these are open attacks on what the author considered false doctrines and superstitions, and no attempt is made to veil their manifest tendency. The style adopted by ROBERTUS DE NOBILIBUS is remarkable for a profuse intermixture of Sanskrit terms; these, to express doctrinal notions and abstract ideas, he compounds and recompounds with a facility of invention that indicates an intimate knowledge of the language whence they are derived; and there can be no doubt, therefore, that he was fully qualified to be the author of these writings. If this should be the fact, considering the high character he bears, and the nature of his known works, I am inclined to attribute to him the composition only, not the forgery,† of the pseudo-Vedas." Mr ELLIS supposes that, after the substance of these works had been composed by ROBERTUS DE NOBILIBUS, they were re-arranged, furnished with introductions and titles, copied in the Roman character, and translated into French, by some other person. But whoever may have put these so-called Vedas into their existing shape, there appears to be no doubt whatever that they were originally composed by a European missionary. And even if we were to suppose that they were composed by a native scholar under a missionary's superintendence, this fact alone would more than suffice to prove that the existence of the Sanskrit language and literature was well known to the members of the Roman missions at the early period in question.

The systematic and continuous study of Sanskrit by Europeans dates, how-

* ROBERT DE NOBILIS, a near relation of Pope MARCELLUS II., and nephew of Cardinal BELLARMIN, founded the Madura Mission about the year 1620.

† Dr MILL does not agree with Mr ELLIS in exonerating ROBERTUS DE NOBILIBUS from all share in the forgery, inasmuch as he (or the original author, whoever he was) puts Christian sentiments into the mouth of Hindu sages.—Preface to "Christa Sangita," p. vii.

ever, only from the last quarter of the last century, when it began to attract the notice of Englishmen in Calcutta. In the course of the inquiries instituted by British statesmen, immediately after the conquest of Bengal, into the condition of the remarkable people who had fallen under English sway, and into the means of promoting their good government and welfare, attention was speedily directed to their sacred language and literature, as the main source from which authentic and satisfactory information must be drawn in regard to their laws and institutions; and more especially, as the Government had adopted the equitable principle that the civil rights of the Hindus should be adjudicated according to their own laws, it was soon perceived to be necessary that these laws should be studied in the original sources. The first step taken for this purpose was to procure a compilation from the principal institutes of Indian jurisprudence, which was drawn up by native jurists. This was next translated into Persian, at that time the language of public business in Bengal; and from this version an English translation was formed by Mr HALHED, and published in 1776. Not content with knowledge thus obtained at second hand, Sir WILLIAM JONES, Judge of the Supreme Court of Judicature, who had been distinguished before his arrival in India by his attainments in other branches of Oriental literature, devoted himself to the study of Sanskrit, and rendered the Institutes of Manu from that language into English. This work was published at Calcutta in 1794. The Indian studies of Sir WILLIAM JONES were not, however, by any means confined to the subject of jurisprudence, but extended to Sanskrit literature generally; and he was the first to draw attention to the Indian drama, one of the most beautiful productions of which, the Sakuntalā, he translated. This translation was published in 1789. The Bhagavad Gitâ, a philosophical, or rather a theosophic, episode from the great epic poem the Mahabharata, had been published at even an earlier date by Mr, afterwards Sir CHARLES, WILKINS, to whom we also owe a valuable Sanskrit grammar, which appeared in 1808.*

The next great name in Sanskrit literature is that of Mr H. T. COLEBROOKE, who made many most important contributions to our knowledge of the Vedas, of Hindu law, poetry, algebra, astronomy, and finally, of the Indian philosophical systems. These contributions, to some of which I shall have occasion to return further on, were commenced in 1795, and continued till 1827.† The late Mr H. H. WILSON, Boden Professor of Sanskrit in the University of Oxford, first came

* The first grammar of Sanskrit in a European language was published by a Carmelite monk, JOHANN PHILIP WESDIN, better known as PAULINUS A SANTO BARTHOLOMÆO, at Rome, in 1790. See MÜLLER'S "Lectures on the Science of Language," p. 149; also Professor WILSON'S paper in the "Proceedings of the Philological Society," vol. i. p. 16, where it is said that a second, more copious and correct, grammar was published by PAULINUS n 1804.

† For an account of Mr COLEBROOKE'S career, see the Notices of his Life by his son, Sir E. T. COLEBROOKE, the present M.P. for Lanarkshire, in the Journal of the Royal Asiatic Society, No. IX., for August 1838.

before the public as an Orientalist in his translation of the Meghadūta, a poem of Kālidāsa, in 1813. He subsequently compiled an extensive and most valuable Sanskrit-English Dictionary, of which the first edition appeared in 1819, and the second, greatly enlarged and improved, in 1832. This is the only complete Sanskrit Lexicon which we yet possess; and though it by no means comes up to the modern standard of lexicography, gives but a very imperfect explanation of philosophical terms, and includes few, if any, of the numerous obsolete words which are peculiar to the Vedas, it is yet a work without which Sanskrit studies could never have attained their present flourishing condition. In 1840, Professor WILSON published his translation of the Vishnu Purana, a system of ancient Hindu mythology and tradition. His last great work was his translation of the Rigveda, of which three volumes appeared from 1850 to 1857, while the remainder is still unpublished. He also contributed to different journals a large number of miscellaneous dissertations on various branches of Indian literature, a collected edition of which is now in course of publication.* Among the later English cultivators of Sanskrit I may mention Dr BALLANTYNE, Professor FITZEDWARD HALL (who, though an American, was in the Educational service of the British Government in India), Professor MONIER WILLIAMS, and Mr C. B. COWELL.

Meanwhile, the new language and literature which had been brought to light in the East, had from an early period in this century attracted the attention of Continental scholars, who found in it a most congenial object of study, and an ample field for the exercise of their industry and acuteness. Of these the most eminent names are those of CHEZY, BURNOUF, and REGNIER in France; the SCHLEGELS, BOPP, LASSEN, ROSEN, STENZLER, BÖHTLINGK, ROTH, BENFEY, POTT, MAX MÜLLER, GOLDSTÜCKER, WEBER, AUFRECHT, HAUG, and others in or of Germany; and WESTERGAARD in Denmark. I must also add to these foreign names that of Professor WHITNEY in America.

I think I am doing no injustice to our own countrymen when I say that by far the greatest and most important advances which have of late been made in our knowledge of Indian antiquity are due to Continental scholars. It is to BOPP that we owe the Comparative Grammar of the Indo-European tongues, in which the relations of Sanskrit to the languages of the West are exhibited, and the principles of comparative philology are expounded. It was BURNOUF of Paris who led the way to an accurate knowledge of the Zend language and literature, and who, by a laborious study of the voluminous MSS. sent to Europe by Mr B. H. HODGSON, first threw a clear light on the rise of Buddhism, and on the history and doctrines of its founder.† It was the young German scholar ROSEN

* For an account of Professor WILSON's career, see the Annual Report of the Royal Asiatic Society for 1860, pp. ii. ff. in the Journal of the Society, vol. xviii. part i.

† For a sketch of BURNOUF's labours, see the Annual Report of the Royal Asiatic Society for 1853, in vol. xv. part i. of the Society's Journal.

who first pointed out the importance of a careful study of the Vedas for a correct insight into the early history of the Hindus, and by his translation of the first portion of the hymns prepared the way for the subsequent researches by which his countrymen ROTH, BENFEY, MÜLLER, and AUFRECHT, have begun to elucidate the true meaning and significance of those ancient records.*

It is with the Vedas that the most eminent foreign scholars have of late years chiefly occupied themselves; and there is no doubt that they have acted wisely in giving this direction to their researches, as a knowledge of these ancient works is indispensable to the right comprehension of all the later developments of the Hindu mythology and institutions.

Having thus briefly noticed the earlier stage of Sanskrit studies, I shall now give some account of their more recent advances.

As it is mainly from an examination of the Sanskrit language, when compared with the classical tongues of the West, that we are able to form any conception of the earliest history of the Hindus, I shall first say something of the language. It is thus characterised by Sir W. JONES, in words which have been often repeated:—

"The Sanskrit language," he says, "whatever may be its antiquity, is of a wonderful structure; more perfect than the Greek, more copious than the Latin, and more exquisitely refined than either; yet bearing to both of them a stronger affinity, both in the roots of verbs and in the forms of grammar, than could have been produced by accident; so strong that no philologer could examine all the three without believing them to be sprung from one common source, which perhaps no longer exists."

We need not inquire whether or not the Sanskrit is really, as Sir W. JONES affirms, "more perfect than the Greek." But his assertion that the former language possesses a strong affinity, both in roots and forms of grammar, to the Greek and Latin, is one which has been fully verified, and rendered more positive, by later researches. The investigations of Professor BOPP and his school have now shown that the affinity extends to a large number of roots and forms of inflexion, between which no mutual relationship had been at first suspected to exist. By tracing the different steps by which words have, in the long course of ages, been gradually modified, and by pointing out the laws according to which certain letters vary in these three languages respectively (*i.e.*, how, in some cases, a particular letter in Sanskrit is always or commonly represented by such another letter in Greek, and by such a third letter in Latin), these scholars have been able to establish the affinity of numerous words, the connection of which had been previously obscured by their apparent dissimilarity. And not only has it

* See the Notice of his Life prefixed to his edition, and Latin translation, of the first book of the Rigveda. London, 1838.

thus been made clear that the connection of Sanskrit with Greek and Latin is even more intimate than had at first been imagined, but it has been further shown that these three languages are united to the old Persian, the Teutonic, the Slavonian, and perhaps the Celtic, tongues, by radical and structural resemblances which prove that they all belong to the same stock.

But, as a rule, affinity in language proves affinity in race. It is true that this proposition does not hold good in all circumstances. It sometimes happens that a superior and conquering race imposes its language on an inferior nation, which thenceforward employs a form of speech either entirely or partially distinct from that of its ancestors. But there is no reason to suppose that those tribes, themselves warlike, enterprising, and comparatively civilised, which conveyed to India, to Persia, and to the shores of the Mediterranean, respectively, the earliest forms of the Sanskrit, Persian, Greek, and Latin dialects, were at the commencement of their history subjected to any such action of alien races as could have robbed them of their hereditary languages. We may therefore presume, with every appearance of reason, that these several tribes carried with them to their new homes the same forms of speech as had been employed by their forefathers, with no greater modifications than are wrought in all languages by the lapse of time. Assuming, then, in the absence of any evidence to the contrary, that, in the case before us, affinity in language proves affinity in race, we may fairly conclude that the Sanskrit-speaking Hindus, the Persians, the Greeks, the Romans, the Teutonic and Slavonian nations, are all descended from the same stock. The common ancestors of these various races must of course have spoken one common language, now long extinct, from which the several cognate idioms, Sanskrit, old Persian or Zend, Greek, Latin, and so forth, were offshoots. By no other supposition than this of such common ancestors, using such a common form of speech, can we account for the relations which we find to exist between those different languages. The country occupied by the common ancestors of the nations in question appears to have been Central Asia. This is the conclusion to which the most intelligent scholars have been conducted by considering the positions which the different races speaking the cognate languages severally occupied at the dawn of history. As at that early period we discover the Sanskrit-speaking race in India, the Zend-speaking race in Persia, the Greeks and Latins in Southern, and the Teutonic tribes in Central Europe, it is most natural to suppose that these various nations radiated, in the direction of their several homes, from a central point more or less nearly equidistant from those different countries; and this condition is best fulfilled by Central Asia, the region which I have named.

This theory, when applied to the forefathers of the Hindus, implies that they immigrated into India from the north-west; and this conclusion is strongly confirmed by the fact that the geographical references in the oldest part of Indian literature, the hymns of the Rigveda, indicate that at the early period when they

were composed, the Indian people were dwelling in the north-western part of India, the Punjab, while the later literature enables us to trace their gradual progress from that position onward to the east and south.

Sanskrit is found in its oldest known form in the Rigveda, the hymns of which are with good reason conjectured to have been composed during a period not less than from 1200 to 1400 years B.C. The language, as it is exhibited to us in these hymns, is characterised by Mr COLEBROOKE (Essays, i. 309) as "a rustic and irregular dialect;" and differs in its grammatical forms from the later Sanskrit (as it existed in its ultimate development above two thousand years ago) much more than Homeric does from Attic Greek. In the Brāhmanas, the class of works which follow next in order of time after the Vedic hymns, but which are removed from the oldest of them by an interval of several centuries, we find that the language has changed, and approaches considerably nearer to the later Sanskrit. At the period when the hymns were composed, there is no doubt that Sanskrit was a spoken idiom.* For, first, it is inconceivable that in

* In the present state of philological science, the speculations of DUGALD STEWART on the origin of the Sanskrit language are not likely to meet with many defenders; but as the views of this eminent philosopher must always possess a certain interest in the country where he so long flourished, and attained so high and well deserved a reputation, I shall briefly allude to his theory on this subject. His supposition is (see Sir W. HAMILTON's coll. ed. of his works, vol. iv. pp. 77–115; and vol. i. pp. 425-427) that "in consequence of the intercourse between Greece and India, arising from Alexander's conquests, the *Bramins* were led to invent their sacred language," for the purpose of expressing the new ideas which they had received, and of concealing "from the other Indian castes their philosophical doctrines, when they were at variance with the commonly received opinions" (p. 83). He thinks that "with the Greek language before them as a model, and their own language as their principal raw material," they could have no difficulty in "manufacturing a different idiom, borrowing from the Greek the same, or nearly the same system, in the flexions of nouns, and conjugations of verbs, and thus disguising, by new terminations and a new syntax, their native dialect. If Psalmanazar was able to create, without any assistance, a language of which not a single word had a previous existence but in his own fancy, it does not," STEWART proceeds, "seem a very bold hypothesis, that an order of men, amply supplied with a stock of words applicable to all matters connected with the common business of life, might, without much expense of time and ingenuity, bring to systematic perfection an artificial language of their own, having for their guide the richest and most regular tongue that ever was spoken on earth; a tongue, too, abounding in whatever abstract and technical words their vernacular speech was incompetent to furnish" (p. 84). He also explains that although he had supposed "the first rude draught of the Sanskrit to have been formed soon after Alexander's invasion had introduced the learned in India to an acquaintance with the Greek language and philosophy, this supposition was not meant to exclude other languages from having contributed their share to its subsequent enrichment. The long commercial intercourse of the Romans with India, both by sea and land, accounts sufficiently," he considers, "for any affinity which may subsist between Sanskrit and Latin." The arguments by which STEWART endeavours to sustain this theory may be consulted by the reader for himself. The whole discussion furnishes a curious illustration of the mistakes into which acute and ingenious men may be betrayed when they attempt to theorize upon subjects with the details of which they are imperfectly acquainted, and at a period when the principles which ought to govern the investigation have not been sufficiently developed or recognised. It appears from a note of his editor, p. 115, that Mr STEWART subsequently elaborated his speculations on Sanskrit in a treatise which he left in manuscript, but which Sir W. HAMILTON did not think it right to publish, as the hypothesis, however ingeniously supported, was contrary to the "harmonious opinion now entertained by those best qualified to judge," and beset with many "difficulties appearing insuperable." STEWART's views have been discussed by the late Professor H. H. WILSON, in a paper which is to be reprinted in the new edition of his works now in

that early age a learned and sacred language should have existed alongside of the current vernacular dialect. In order to fulfil their intended purpose, as expressions of the devout sentiments of the worshippers who used them, the hymns must have been intelligible not only to the priests and chiefs, but also to the people in general. And, secondly, it is only by supposing that the Sanskrit of the hymns was a spoken idiom that we can account for the gradual modifications which it afterwards underwent. At length, however (at a period which cannot now be settled with any precision, but which must be placed several centuries before the beginning of our era), Sanskrit ceased to be spoken, and it continued thenceforward fixed in the form which it had at that period taken, and in which it was exhibited in the standard works of that age. The vernacular dialect by which it was at first succeeded, arose, no doubt, gradually, out of popular modes of pronunciation, and differed little from the original language, of which it preserved, with some modifications, the principal forms of declension and conjugation. This we see in the earliest Indian vernacular idiom which has come down to us, the Pali, which, as it was the prevailing form of speech at the time when the founder of the Buddhist religion flourished, and was employed by him for the propagation of his tenets among his countrymen, has been preserved in the sacred books of that religion discovered in Ceylon. This dialect differs from Sanskrit very much in the same way as Italian differs from Latin, and many of the complex sounds of the Sanskrit are softened down in Pali in precisely the same manner as similar sounds in Latin are modified in Italian. Thus the Sanskrit words *muktas*, *bhuktas*, *suptas*, *guptas*, become in Pali *mutto*, *bhutto*, *sutto*, and *gutto*, just as the Latin *perfectus*, *fructus*, *ruptus*, *actus*, become in Italian *perfetto*, *frutto*, *rotto*, *atto*; and so on in a number of other parallel cases. The Pali was followed by various provincial dialects, called Prākrits, which continued to diverge more and more from the Sanskrit, and from one another,—gradually dropping the modes of inflection proper to the parent tongue, and supplanting them by separate connecting particles,—till at length they issued in the modern vernacular idioms known as the Hindi, Bengali, Gujarāti, Marathi, &c. The languages of the south of India—Tamil, Telugu, Canarese, Malayālim—belong to a quite distinct family. They are not, like the north Indian vernacular tongues, derived from Sanskrit, but are (especially the most pure and perfect of them, the Tamil) completely inde-

course of publication by Messrs Trübner & Co. See also the Quarterly Review, vol. liv. p. 299. On this theory I may shortly remark, 1*st*, That unless the views of all the most competent scholars, such as COLEBROOKE, WILSON, MAX MÜLLER, GOLDSTÜCKER, AUFRECHT, &c., in regard to the age of the Vedic hymns, are grossly erroneous, the Sanskrit language must have existed for at least eight hundred or a thousand years before Alexander invaded India; and this language, as it existed at that early period, exhibits, in quite as distinct a manner, all those affinities in roots and structure, with Greek and Latin, by which it was distinguished at a later period. 2*dly*, That the vernacular language—which, according to the best evidence we can obtain, existed at, or even before, the time of Alexander (I mean the Pali)—in its structure presupposes the prior existence of Sanskrit quite as necessarily as the forms of the Italian presuppose a Latin language out of which it arose.

pendent of it, though they have begun to admit Sanskrit words in a more or less altered shape, from the period when the Sanskrit-speaking races first penetrated into the southern parts of the peninsula. From this fundamental difference of language we are led to conclude that the people of the south must belong to a different race from the Sanskrit-speaking inhabitants of the north of India, and that they were probably driven southward by the northern invaders soon after the entrance of the latter into Hindostan from the north-west.

I now pass from the language to the literature. This literature is of great extent, and ranges over a period of three thousand years. It comprises large collections of ancient hymns, liturgical treatises, theological tracts, voluminous legendary and mythological poems, various systems of philosophical speculation, numerous works on grammar, law, medicine, algebra, astronomy, astrology, and a long list of dramatic and other poetical compositions. This literature presents in certain respects a general likeness to that of Greece, but the points of dissimilarity are greater than those of resemblance. Philosophical speculation, mathematical and astronomical science, have been cultivated in both countries. Epic and dramatic poetry, too, are found in India as well as in Greece, though the frequent exaggeration and extravagance of the Hindu conceptions stand in most unfavourable contrast to the harmony and just proportion which distinguish the creations of the Greek imagination. But there is another respect in which the two literatures differ remarkably from one another. In Greece the earliest poetry was mainly epic, whilst in India it was of a lyrical character. Instead of ballads recounting chiefly the exploits of warriors (though without excluding mythology), as in Greece, we find in India large collections of hymns celebrating almost exclusively the praises of the national deities, and destined to be employed in their worship. These hymns are succeeded by ritual treatises prescribing the order of sacrifice, and by theological tracts, for all of which a divine authority is claimed. There is nothing in Greek literature which presents any parallel to this. A further difference is, that whilst in Greece epic poetry comes first in order, is written in the oldest extant form of Greek, and represents to us the Hellenic mythology in its earliest known phase, the same description of poetry appears in India at a different epoch, long after the age of the hymns, and, unlike the latter, is composed in Sanskrit, which, with the exception of a few obsolete grammatical forms, is of the most modern character; while at the same time the mythology and the religious institutions which it brings before us are far more advanced than those which we discover in the hymns. These Indian epics combine in some degree the characteristics of the Homeric poems with those of the theogony of Hesiod, but both of these elements assume in the Hindu works a far greater magnitude, and a luxuriance which runs into extravagance; and in the course of ages, one of them, the Mahābhārata, has swelled, by gradual accretions, to an enormous bulk.

As it would be impossible to compress within any moderate compass an account of the various productions of the Indian intellect which I began by enumerating, I must confine myself to those which are most characteristic and important.

The hymns of the Veda are, as we have already seen, the oldest part of Indian literature.* The word Veda is a general term, including all the most ancient and most sacred of the Hindu writings,—those which they regard as alone inspired and infallible in the proper sense. According to the native definition, the Veda consists of two divisions,—*Mantra* and *Brâhmana*. The Mantras are the hymns, the most original and most essential part of the Veda; while the Brâhmanas are liturgical works which prescribe the various forms of sacrificial worship, and explain the mystical meaning of many of the rites.

There are four collections of hymns, entitled respectively the Rik, Sâma, Yajur, and Atharva-sanhitâs. The principal and most important collection is that of the Rigveda. It consists of about a thousand hymns, which in the 8vo edition of Professor AUFRECHT, printed in the Roman character, fill above 900 pages. Professor MAX MÜLLER, in his lately published history of ancient Sanskrit literature (the most important work in this department which has yet appeared), states the period of the composition of these hymns conjecturally, as ranging from 1200 to 800 years before Christ; and the opinion of most Sanskrit scholars is that this estimate of their antiquity probably falls short of the truth. No competent investigator can for a moment doubt that these hymns are genuine remains of Indian antiquity, and anterior in date not merely to any other of the Hindu writings, but also to any other existing literary monuments of the Indo-Germanic race. This is proved beyond all question, not only by the archaic forms of the language, and by the number of peculiar words, afterwards disused, which they contain, but also by their contents, which represent to us a simple and inartificial condition of society, and display the mythology, and the sacerdotal and religious institutions of the Hindus in their most rudimentary form. The period during which these hymns were composed extends, no doubt, over several centuries. This is shown both by the probabilities of the case, and also by the fact that reference is made in various passages to newer and older hymns, and to different

* I may mention here that the principal authors who have translated or described or edited the Vedas are Mr H. T. COLEBROOKE, who, in 1805, gave a general account of these works, with specimen translations (Essays, vol. i. pp. 9–113); Professor ROSEN, whose Latin version of the first book of the Rigveda was published in 1838; Professor ROTH, who published in 1846 three Dissertations in German on the literature and history of the Veda; Professor MAX MÜLLER, who has published, between 1849 and the present time, four volumes of his edition of the Text and Commentary of the Rigveda, besides a history of ancient Sanskrit literature; Professor H. H. WILSON, who translated the whole of the Rigveda into English, though only half of his translation has yet been published; Professor BENFEY, who has translated the Sâma Veda into German; Professor AUFRECHT, who has published the text of the Rigveda in the Roman character; and Professor WEBER, who has published the text of the White Yajur Veda and the Satapatha Brâhmana.

generations of poets, and that some of the hymns appear to display an advance in thought and speculation, as well as a progress in some other respects, as compared with others. The hymns are composed in a considerable variety of metres, which are not, indeed, always regularly constructed, but which, by their number and occasional elaborateness, form a remarkable contrast to the metrical uniformity of the oldest Greek poetry. In fact, throughout the whole course of Sanskrit literature we see the people who spoke and wrote the language manifesting a most delicate sense of harmonious composition.

The Vedic poetry is simpler, much less flowery in style, and consequently more akin to the productions of Western classical literature,—more consonant, in short, to the common Aryan type on which the Indians of that age were moulded,—than is the case with the later Indian poetry, produced at a period when the Hindus had resided for many centuries in their new abode, and when the various physical influences of a more southerly climate had combined to modify their mental constitution, and impart an oriental luxuriance to their conceptions.

The hymns are, with few exceptions, of a religious character. "In these songs," to use the words of Professor Roth, "the forefathers dwelling on the banks of the five rivers (of the Punjab) supplicated prosperity for themselves and their herds, greeted the rising dawn, celebrated the conflict of the Thunderer with the gloomy Power, and acknowledged the succour of the deities who had delivered them in battle." The hymns are addressed to a variety of gods, in whom the most remarkable powers and phenomena of nature are personified. These are Agni (fire); Indra (the ruler of the firmament, who dispels drought and sends down rain); Savitri (the sun); Vishnu (perhaps another form of the same luminary, who strides across the heavens in three paces); Rudra (who is considered to be the god of tempests, but was at a later period identified with Siva); Varuna (whose name corresponds to the Greek Οὐρανός, and who in the Veda represents the nightly sky, though he came afterwards to be regarded as god of the ocean); Maruts (the winds); Ushas (the dawn), &c. The functions assigned to these different deities are various, and their respective provinces are frequently confounded. Yāska, the oldest extant interpreter of the Veda, thus classifies the gods who are celebrated in the hymns:—"There are," he says, "according to the etymologists, three deities, Agni, whose place is on earth; Vāyu or Indra, whose position is in the atmosphere; and Sūrya (the sun), who occupies the sky. These deities receive many appellations in consequence of their greatness, or of the diversity of their functions, just as one and the same man is called by various names denoting different priestly offices; or the different gods so diversely designated may all be distinct, for the praises addressed to them, and also their appellations, are separate." The same writer, however, regards all these deities as being in reality only manifestations of one supreme Spirit, for he remarks: "From the vastness of the deity, the one Soul is lauded in many ways.

The different gods are respectively members of the one Soul." This mode of conceiving the gods must however be regarded as a subsequent refinement, belonging to an age of reflection; as though some of the later hymns certainly contain passages which express monotheistic, or perhaps rather pantheistic ideas, the independent individuality of the different gods appears to be the doctrine of the older hymns. Some of the hymns (supposed by Professor MÜLLER to be among the latest) sing the praises of certain liberal princes, who had bestowed ample largesses upon their authors. One contains the lamentations of a gambler who had lost his all by infatuated addiction to play; a second seems to consist of a satire on the performances of priests, which it compares to the croaking of frogs at the commencement of the rainy season; others contain some references to conflicts between different chieftains and tribes, and to the contests of different families ofpriests for pre-eminence. (See ROTH on the Lit. and Hist. of the Veda, Diss. 3d.) It is difficult to say whether this collection of hymns has preserved to us all the poetical productions of that early period. On the one hand, it might have been supposed that when so many persons possessed so great a facility in poetical composition, they would have exercised their powers on a greater variety of subjects; and we may suspect that, as the hymns were collected by the sacerdotal descendants of the authors, the collectors may have been less anxious to preserve those which were not of a religious character. On the other hand, it may be urged, that the peculiarly strong influence which religion exerts over the imaginations of men in an early stage of culture, and the comparative triviality of terrestrial affairs, or perhaps the higher profit resulting from the employment of their talents on subjects connected with the worship of the gods, may have led the ancient bards to direct their efforts into this channel by preference. On the whole, it seems most probable that many of the old hymns have been lost.

No particular sanctity appears to have been at first ascribed to the hymns. Their authors, indeed, speak of them, especially of those which were new, as being acceptable to the gods; but they generally describe them as the productions of their own minds,—as being made, fashioned, generated, by themselves; and though references are occasionally found to the aid or suggestion of the different gods, and though in other places a mysterious power is ascribed to the hymns, it does not seem to have been the prevailing idea of the poets that their compositions were the results of any divine inspiration. The descendants of the bards, however, who preserved and collected the hymns, soon began to attribute a peculiar sacredness to the productions of their ancestors, and to regard them as the most efficacious forms in which the favour of the gods could be supplicated. This had long been the received opinion when the Brāhmanas were compiled, as in these works the Vedas are said to have sprung, though only mediately, from the Creator. Even in the Upanishads (which are the concluding portions of the Brāhmanas), however, it does not appear that the same broad line of demarcation

is drawn between the Vedic hymns and the other and later parts of Indian literature as came afterwards to be the case. At a subsequent period a very high and exclusive authority and sacredness were attributed to the Vedas, which were declared to have been only *seen*, not composed, by their apparent authors, the ancient rishis.* The divine character of these books is however placed upon different grounds by writers belonging to different schools of philosophy. Some say that the Vedas have no personal, not even a divine, author, but are uncreated, and owe their authority to their eternal faultlessness; while others maintain that they were consciously uttered by a divine person, whose perfect intelligence is the source of their infallibility.†

The hymns included in the collection of the Rigveda are, as I have observed, of very different dates, and indicate different stages in the development of the Indian intellect and institutions.

I shall now offer some short specimens of these hymns; first, of those which, from the simplicity of their nature-worship, must be considered the more ancient; and afterwards of those which, from the speculative character which they begin to assume, may safely be pronounced to be among the most recent.

The following is the first hymn of the Rigveda, and will serve as well as any other to give an idea of the oldest of these odes. It is addressed to Agni, the god of fire, to whom a variety of functions are assigned, and who, both from the importance of the element which he represents, and from his close connection with sacrifice, holds a prominent place among the deities of the Rigveda:—

"1. I laud Agni the priest, the divine minister of sacrifice, who invokes the gods, and is most rich in gems. 2. Agni has been an object of worship to ancient as well as modern bards: may he bring the gods hither! 3. Through Agni the worshipper attains a prosperity which increases day by day, and is accompanied by renown and by a vigorous progeny. 4. Agni, that oblation, duly presented, which thou envelopest on every side, goes straight to the gods. 5. May Agni, the invoker, the sage, the true, the most renowned, a god, come hither with the gods! 6. Whatever good thou, Agni, shalt do to thy worshipper, that, O Angiras, shall be truly thine own. 7. Thee, O Agni, dispeller of gloom, we approach every day with our hymn, offering adoration. 8. (Thee we approach), shining protector of ceremonies, the brilliant (guardian) of sacred rites, ever waxing in thine own abode. 9. Be thou ever accessible to us, Agni, as a father to his son: be present with us for our good."

The verses which I shall next quote are from a hymn to Indra, the god of the firmament, who, as one of his chief functions, is conceived as smiting Ahi or Vrittra, the cloud-demon, the withholder of rain, and as compelling him to discharge his watery treasures on the thirsty earth:—

"1. I declare the heroic deeds of Indra, which the thunderer achieved of old. He smote Ahi, he discharged the waters, he let loose the torrents of the aerial hills. 2. He smote Ahi, resting upon the mountain. Tvashtri (the Indian Vulcan) forged for him his swift-darting thunderbolt. Like lowing kine, the rushing waters hastened to the ocean. 3. Impetuous as a bull, he sought

* See my "Original Sanskrit Texts," vol. iii. pp. 90; 110, ff.
† Ibid. pp. 52, ff; 196, ff; 212, ff.

the soma*-draught, he quaffed that which had been poured forth into three cups. He seized the thunderbolt for his shaft; he smote him, the first-born of the Ahis. 4. When thou, Indra, smotest the first-born of the Ahis, thou didst vanquish the magic of the magicians. Producing then the sun, the sky, the dawn, thou foundest no longer any enemy remaining. 5. With his mighty shaft, the thunderbolt, Indra smote in pieces the gloomy Vrittra. Like the trunk of a tree felled with the axe, so lies Ahi prostrate upon the earth. 6. Like a weak but foolhardy warrior, he challenged the impetuous hero, the destroyer of many; but he did not escape the stroke of his bolts. Encountered by Indra, he crushed the rivers (by his fall)," &c. &c.

I shall now give a few verses as a specimen of a hymn to Ushas or Aurora, some of which (as Professor MÜLLER remarks†) might naturally be uttered by a poet still, after the lapse of 3000 years:—

" 7. She, the daughter of the sky, has been beheld, breaking forth, youthful, clad in shining garments, mistress of all earthly treasures. Auspicious Ushas, shine on us here to-day! 8. Ushas follows the track of the dawns that are past, and is the first of the unnumbered dawns that are to come, breaking forth, arousing life, and awaking every one that was dead. 9. Inasmuch as thou hast made the fire to be kindled, hast unveiled all things by the light of the sun, and hast awakened the men who are to offer sacrifice, thou hast done a good service to the gods. 11. Those mortals are gone who saw the earliest Ushas dawning: we are to behold her now; and the men are coming who shall gaze upon her on future morns. 12. Perpetually, in former days, did the divine Ushas dawn: to-day, too, this opulent goddess has revealed the world. Still shall she appear on future days: undecaying, immortal, she marches on by her own will. 14. She hath shone forth with her splendours in the regions of the sky; the bright goddess has manifested the dark form of night: awakening the world, Ushas advances in her car, drawn by ruddy steeds. 16. Arise: our life, our breath has come: darkness has departed, light arrives: Ushas hath opened up a path for the sun to travel. We have arrived there where men prolong their days. 19. Mother of the gods, manifestation of Aditi, revealer of the sacrifice, mighty Ushas, shine forth! Arise, conferring renown upon our hymn," &c.

The next verses are from one of the later hymns in the Tenth Book:—

" 1. Hiranyagarbha‡ arose in the beginning: he was the one born lord of things existing. He established the earth and this sky. To what god shall we present our offering? 2. He gives soul, he gives strength. His command all, even the gods, obey. His shadow is immortality, his shadow is death. To what god shall we present our offering? 3. He by his might became the one king of the breathing and winking world: he is lord of this two-footed and four-footed creation. To what god shall we present our offering? 4. By his might, they say, these snowy mountains arose, and the ocean, with the river, was produced: these aerial regions are his arms. To what god shall we present our offering? 5. He made the sky fiery, and the earth fixed; he established the firmament and the heavens; he in the atmosphere measures the aerial spaces. To what god shall we present our offering?"§

The last specimen of the Rigveda which I shall offer is also from the Tenth Book, where we see the mind of the poet striving to form for itself a theory of creation:—

" 1. There was then neither nonentity nor entity. There was no atmosphere, nor sky beyond. What covered all? What was the receptacle of each thing? Was it water, the profound abyss? 2. Death was not then, nor immortality: there was no distinction of day or night. That One breathed tranquil, by its own power. There was nothing different from or beyond it. 3. There was darkness, originally enveloped in darkness: this universe was undistinguishable water. That

* Soma is the moon-plant, the juice of which was offered to the gods, whom it was supposed to exhilarate.

† Anc. Sansk. Lit. p. 551.

‡ One of the names of Brahmā, the creator.

§ Translated in MÜLLER's Anc. Sansk. Lit. p. 569.

which, a void, was covered with nothingness, sprang into being as One, by the power of heat. 4. Desire was first produced in it, which is the first germ of mind. The wise, searching in their heart, have, by their intellect, discovered in nonentity the bond of entity. 6. Who knows, who here can declare, whence has sprung, whence, this development? the gods are subsequent to the evolution of this universe: who then knows whence it arose? 7. From what source this development arose, and whether any one created this universe or not, he who in the highest heavens is its ruler, he truly knows, or he does not know."*

An English translation of about half of the hymns of the Rigveda has been published by the late Professor H. H. WILSON, who follows the interpretation given by the native commentator SĀYAṆA. Though there is no reason to doubt that this commentator has in general rendered the sense of the hymns with tolerable correctness, it is scarcely less clear that, in regard to a large number of words and phrases, his explanations are not satisfactory. The hymns, as I have said, are separated by a wide chasm from all the later literature. Numerous words which occur in them are found nowhere else; and the tradition of the true meaning appears to have been lost. Though the Brāhmanas, if thoroughly explored, would, according to Professor MÜLLER,† supply most valuable materials for the explanation of old Vedic words, yet many difficult terms would remain, of which they would offer either no satisfactory interpretation, or none at all. The Brāhmanas are not commentaries. Their primary object is to prescribe the details of particular ceremonies, and the manner in which the texts of the Vedic hymns are to be recited; and though, in the course of their directions, they introduce many etymologies, these are frequently fanciful, and accompanied by mystical references. There is indeed one extant list of Vedic words partly obsolete, and a commentary on them by YĀSKA, called Nirukta, in which the author gives his interpretation of many of the more difficult words in the Rigveda. This writer is considered to have lived several centuries before Christ; but as his interpretations are always accompanied by etymologies, and are sometimes alternative, it would seem as if the true sense of many of the words which he expounds had been already lost in his day, and even then could only be recovered by resorting to the meaning of the roots with which they seemed to have most affinity. But if such was the case at that early period, how much more difficult must the explanation of the hymns have become in the age in which the commentator SĀYAṆA wrote (the fourteenth century A.D.), when the Indian mythology, institutions, and manner of thinking, had departed so much more widely from the Vedic standard than it had in the time of YĀSKA.

It should not therefore surprise us, that writers trained in the modern school of philology and interpretation should be unwilling to regard SĀYAṆA's interpretations as satisfactory and final; and we accordingly find that BENFEY, ROTH, MÜLLER, AUFRECHT, and others, while they derive from the native commentators

* See MÜLLER's fine remarks on this hymn, and the metrical version given by him, in Anc. Sansk. Lit. p. 559, ff.

† Anc. Sansk. Lit. p. 153, f.

all the aid which they can afford, and which indeed is indispensable, have yet been endeavouring to go farther, and to extract from the hymns themselves, studied etymologically, and with reference to all the parallel passages, as well as to the general ideas of the Vedic age, the true sense which the ancient authors themselves intended to convey, uncoloured by any of the conceptions of more modern Indian expositors.

In the valuable Sanskrit and German Lexicon of Messrs Böhtlingk and Roth, published at St Petersburg under the auspices of the Imperial Russian Academy of Sciences (of which about half has now appeared), an attempt is made to ascertain the true meaning of the ancient Vedic words, as well as to arrange and interpret the other component parts of the Sanskrit language on scientific principles.*

Besides the collection of hymns entitled Rigveda-Sanhitâ, there are, as we have seen, three others. Of these, the Sāma-veda consists, with few exceptions, of verses extracted from hymns in the Rigveda, generally detached from their contexts, and arranged for liturgical purposes. This Veda is consequently of but little importance, except for a few various readings which it offers. The Yajurveda consists partly of liturgical formulas and invocations in prose, and partly of verses, some of which occur also in the Rigveda. Some of the contents of the Yajurveda are valuable, as enabling us to trace the history of the Indian deities, especially Rudra. In the fourth or Atharvaveda, a considerable portion is extracted from the Rigveda, but by far the larger part is peculiar to itself, and later in date than most of the Rigveda. Much of it consists of formulas for averting evils, chiefly of a physical kind, and for securing prosperity and happiness.

I now come to the Brāhmanas, which, as we have seen, constitute the second division of each Veda, and which, of all the Indian writings, are those that, in point of age, come nearest to the hymns.

For some time after the Aryan Indians had immigrated into Hindustan, their priests do not seem to have formed a distinct caste. Though in one of the latest hymns of the Rigveda, the four castes are specified; and though in another a description is given of the benefits which accrue to a king from employing a priest, yet in most of those which on other grounds appear to be the earliest, no distinct reference is made to any caste-organization; while the hymns of the entire third book are, in the ancient indices, ascribed to Visvāmitra, who was not of sacerdotal, but of kingly descent, and to his descendants, as the authors. At this period, too, the ceremonial of sacrifice was no doubt much simpler, and less fettered by formal regulations than in after times. But when, in the natural pro-

* Another important work in the department of Sanskrit lexicography is the Dictionary which Professor Goldstücker is compiling on the basis of Wilson's, but with great additions and improvements. Of this work five parts have already appeared.

gress of society, the community became more divided into distinct professions, when the ceremonial of sacrifice became more complex and minute, and every deviation from the prescribed forms came to be regarded as fatal to the efficacy of an observance,—it was natural that the priests should then be organised into a separate profession, and that that profession should finally grow into a caste. It was at this stage that the Brāhmanas would be composed, to perpetuate the regulations which had been adopted by the different schools of priests, who, however, it is to be observed, did not by any means always coincide in the details of their ceremonial prescriptions. The object then of the Brāhmanas is to regulate the course of the various sacrificial rites. Thus, the Satapatha Brāhmana of the White Yajurveda prescribes how the sacred fire is to be kindled,—how atonement is to be made for errors committed in the ceremonial,—how the asvamedha or horse-sacrifice, the universal sacrifice, the offerings to the dead, &c., are to be performed. In carrying out their main objects, the Brāhmanas, besides supplying, as I have said, many etymologies and mystical interpretations, introduce also a variety of legends relating to the birth and history of the gods, the origin of all things, &c., with the view of illustrating the efficacy of particular ceremonies. As these books are intermediate in age between the hymns of the Veda and the later mythological works called Itihāsas and Puranas, so also the myths and legends which they (the Brāhmanas) relate, are found generally in a state intermediate between that in which they are seen in the hymns and in the Puranas—*i.e.*, more developed than in the former, and less developed than in the latter. The Brāhmanas thus enable us to trace more accurately than we should otherwise be able to do, the progress of Indian mythology. I will give here one very interesting specimen of the legends narrated in these works. It contains the oldest version of the story of the deluge which is found in any of the Indian books:—

"They brought to Manu (the progenitor of the Aryan Indians) in the morning water for washing, as men are in the habit of bringing water to wash with the hands. As he was thus washing, there came into his hands a fish, which said to him, 'Preserve me, and I will save thee.' 'From what,' asked Manu, 'wilt thou save me?' 'A flood,' replied the fish, 'shall sweep away all these creatures; I will rescue thee from that. 'How,' inquired Manu, 'can I preserve thee?' The fish answered, 'So long as we are small, we are in great peril, and even fish devours fish; preserve me first in a jar. When I grow too large for the jar, dig a trench and preserve me in that. When I become too large for the trench, carry me to the ocean: I shall then be beyond the reach of danger.' Straightway it became a great fish, for it grew exceedingly. The fish then said, 'In so many years the flood will come; make a ship, therefore, and worship me; when the flood rises, embark on the ship, and I will deliver thee.' Manu accordingly preserved the fish, and brought it to the ocean. In the same year which the fish had indicated as that of the flood, Manu built a ship, and worshipped the fish. When the flood rose he entered into the ship. The fish swam near him, and he fastened the cable of the ship to the fish's horn. By this means he passed over this Northern Mountain (the Himâlaya). The fish then said, 'I have delivered thee; fasten the ship to a tree. But lest the water should abandon thee on the mountain, thou shalt descend after it as fast as it subsides. He descended accordingly as the water subsided. Hence, also, this was 'Manu's descent' from the Northern Mountain. Now the flood had swept away all these creatures. Manu alone was left here. Desirous of offspring, he worshipped, and performed a laborious rite. . . . . Thence (from the material of the sacrifice), in a year a female was born," who called herself Manu's daughter: "with her Manu worshipped, desirous of offspring, and with her he begot this race, which is called the offspring of Manu."

In the later version of the same legend, which is given in the great epic poem, the Mahābhārata, there are one or two traits by which the narrative is brought into nearer accordance with the Semitic form of the legend as given in the book of Genesis. These traits are, that Manu is represented as taking the seven rishis or sages, and the seeds of all things, with him into the ship. The late distinguished orientalist, BURNOUF of Paris, was of opinion that the story of the deluge (which, however, he then only knew in the form in which it is found in the Mahābhārata and later works), does not fit well into the system of Indian Puranic chronology, and must for that reason, and also because no historical event is to be found in the Indian records which could have given rise to such a legend,—have been introduced into India from some Semitic source.* This view is contested by Professor WEBER of Berlin, from whose remarks (*Indische Studien*, i. 161 f.) it results that the discovery of the older form of the story, as given in the Brāhmana, and the proof thus obtained of its existence before the Indian system of chronology was elaborated, give strength to the supposition that it had formed part of the most ancient Indian traditions.

I have spoken of the Veda as divided into two parts—*Mantra* and *Brāhmana*, or hymns and liturgical treatises. There is another division, according to which the Vedas are described as having a ceremonial or exoteric part (*karma-kāṇda*), and a spiritual or esoteric part, relating to the knowledge of soul (*jnāna-kāṇda*). This distinction is thus stated in the Muṇdaka Upanishad (one of the theological treatises which are appended to the Brāhmanas), which at the same time clearly expresses the inferiority of the ceremonial division to the spiritual. "Two sciences," says the author, "are to be known (according to sages acquainted with Brahma), the superior and the inferior. The inferior consists of the Rigveda, the Yajurveda, the Sāmaveda, the Atharvaveda, accentuation, ritual, grammar, commentary, prosody and astronomy. The superior science is that by which the imperishable is apprehended." It will be observed that notwithstanding their great sacredness, the hymns, together with the earlier or ritual part of the Brāhmanas, are here pronounced to be of very subordinate value compared with the concluding portion of the Veda, in which the knowledge of soul is unfolded. Ritual observances, though regarded as preparing the way for higher studies, are, according to the highest Hindu doctrine, considered as in themselves leading only to a state of fruition in paradise, which though of a long duration, corresponding to the merits by which it has been earned, is yet temporary, as well as essentially unsatisfying, and inconsistent with that full spiritual perfection to which the enlightened sage aspires as the highest and only enduring condition of future blessedness.

The portions of the Veda in which the means of attaining this final libera-

* See the preface to the 3d volume of his edition and translation of the Bhāgavata Purāna, (Paris, 1847), pp. xxxii–liv.

tion from ignorance and from all mundane imperfections are expounded, are called Upanishads, or Vedāntas (*i. e.*, concluding parts of the Vedas). These tracts contain a variety of speculations, primitive and unscientific both in form and substance, on the origin of all things, on the nature of the human soul, its relation to the supreme Spirit, on the character of that contemplation by which it may attain perfection, and on other kindred topics. The beginnings of this sort of inquiry are to be found, as we have already seen, in some of the hymns of the Veda. In the Upanishads, these questions are pursued at greater length, and with a wider variety of details. I shall give a specimen of one of the best of these works.

The Katha Upanishad represents the Brahman Vājasrăvăsa as having in a fit of anger given up his son Nachiketas to Yama, the Indian Pluto. Nachiketas proceeds to the abode of Yama, and as the latter improperly neglected his Brahman guest, leaving him for three nights unprovided with food, he offers him as a compensation three boons. The youth chooses, as the first, that he may recover his father's favour, and see him again; as the second, that he may ascend to paradise, become immortal, and be made acquainted with the celestial fire. When called upon to choose the third boon, Nachiketas replies:—"Some say a a man exists after death; others say he does not: in regard to this question I desire to be instructed by thee." Yama answers: "This question has of old formed the subject of discussion even among the gods, for it is hard to determine. This principle of being is very subtile. Do not insist on a reply; relinquish this boon and choose another." Nachiketas answers: "Thou sayest, O Death, that this question was discussed even by the gods, and that it is hard to determine. But there is no other who can resolve it like thee, neither is there any other boon comparable to this." "Choose," rejoins Yama, "sons and grandsons who shall live a hundred years; choose cattle, elephants, gold, horses; choose a wide territorial domain; and live thyself as many centuries as thou listest. Or, choose such boons as are unattainable in this world of mortals, celestial nymphs, with cars and musical instruments; but do not inquire into (the secret of) death." Nachiketas, however, rejects all these offered blessings and pleasures, as transient and unsatisfactory, and persists in demanding an answer to his inquiry, as the only boon he cares to accept. Yama then replies: "The good is one thing, the pleasant is another. These two things, concerned with different objects, attract mankind. It is well with him who chooses the good, but he who prefers the pleasant, fails in his aim. The good and the pleasant both solicit men. The wise man considers and distinguishes them. He prefers the good to the pleasant, while the fool chooses the pleasant, because it promotes his bodily ease. Thou, O Nachiketas, having pondered those objects of desire which are pleasant and beautiful, hast relinquished them. . . . These two things, ignorance and knowledge, are widely opposed and discrepant. I know thee, Nachiketas, to be

desirous of knowledge, because many objects of desire did not distract thee. Men living in ignorance, but fancying themselves wise and learned, go wandering about deluded, like blind men led by the blind. Futurity does not become manifest to the fool who is careless, and deluded by wealth. He who imagines that this is the only world, and that there is no other, falls again and again under my (Death's) dominion. There are many who have not heard of soul, and many who have heard of it, and yet have not known it. Wonderful is he who declares it, skilful is he who attains it, marvellous is he who knows it, when instructed by a sage." Yama then shortly after proceeds thus:—"Concentrating his thought upon the supreme Soul, and thus conceiving the Deity who is hard to be perceived, who is withdrawn into mystery, who abides in the cavity of the heart, in inaccessible recesses, and who exists of old, the wise man abandons joy and grief. Having heard and embraced this, having by effort attained the subtile substratum of qualities, a mortal rejoices, having obtained a source of gladness." The soul is characterised as follows:—"The intelligent (soul) is not born, nor does it die. It does not spring from any other source, nor does any one begin to be. Unborn, eternal, everlasting, primeval, it is not slain when the body is slain. If the smiter thinks to slay, or the smitten thinks that he is slain, both are ignorant; the soul neither slays, nor is slain.* The soul, which is minuter than the minutest, and greater than the greatest, is seated in the cavity of this living being. He who is free from desire and grief beholds this greatness of the soul by the grace of the Creator. Sitting, it travels far; sleeping, it reaches everywhere. Who but I should know this deity, who both rejoices and rejoices not? . . . . This soul is not to be attained by interpretation, nor by understanding, nor by much Scripture. It is to be attained by him whom it chooses. This soul chooses that man's body for its own. He who has not ceased from wickedness, who is not tranquil, whose mind is not concentrated, cannot by knowledge attain the soul."

In the next section a simile occurs which has been compared with the well-known image in the Phædrus of PLATO. "Know that the body is a chariot, the soul its master, the intellect his charioteer, and the mind† the reins. The senses are the horses, and their objects the roads. When a man is destitute of knowledge, and his mind is never concentrated, his senses are uncontrollable, like the vicious horses of the charioteer. But when a man possesses knowledge, and his mind is always concentrated, then his senses are submissive, like the charioteer's tractable horses."

We have no means of ascertaining exactly the age of the Upanishads. Though some are of great antiquity, it is easy to perceive, from the contents of others, that they are comparatively modern, and that the antique style which their authors

* Compare the Bhagavad Gîtâ, 2. 19, 20.

† The mind (*mănăs*) is regarded by the Hindus as an internal sense.

affect is merely borrowed to give an authority to their own sectarian tenets, which would not otherwise have been accorded to them.

The beginnings of speculative thought which I have noticed as existing even in the hymns of the Rigveda, as well as in the Upanishads, were destined to receive a wide and varied development in the philosophical schools.* As the history of Indian philosophy has not yet been sufficiently studied, it is difficult to say at what period the doctrines held by the different schools, which were eventually denominated Sānkhya, Nyāya, Vedānta, &c., were first propounded in a definite shape. Sets of concise aphorisms (called in Sanskrit *Sūtras*) constitute, in most cases at least, the oldest form in which the tenets of these systems have come down to us. The aphorisms are statements, for the most part very obscure, as well as brief, of the leading principles of the several schools. Whether themselves at first committed to writing or not, they must have been handed down accompanied by at least verbal explanations from the very date when they were composed. Many of them would not otherwise have been intelligible.

We need not however suppose, that the tenets which these aphorisms expound were all thought out at once, and embodied at the same time in the systematic form in which they now appear. The various dogmas which are now combined as component elements of the different schemes of speculation, had no doubt in many cases existed previously in an isolated form, and were not thrown together till they had been discussed and questioned by the advocates of antagonistic doctrines. The relative antiquity of these different sets of aphorisms, and of the schools which they represent, is not easy to determine, especially as some of them, at least, refer mutually to one another. In the cases where these mutual references occur, it is clear that even those bodies of aphorisms which represent the oldest doctrines, must have been subsequently re-written, or at any rate interpolated in those portions which controvert the principles of the rival schools. That great freedom of speculation must have been permitted in India in ancient times, is evident from a consideration of these aphorisms. The most diverse opinions are there propounded on matters of the greatest importance.

* I may remark, that though Indian philosophy has of late received considerable elucidation from the pens of Dr Ballantyne, Professor FitzEdward Hall, and others, yet the main points had already been given in Mr Colebrooke's Essays, read before the Royal Asiatic Society in 1823 and the following years. See Colebrooke's Misc. Essays, vol. i. 227–419; Dr Ballantyne's "Christianity contrasted with Hindu Philosophy;" his Lectures on the Vedānta, Nyāya, and Sānkhya Philosophies, and his translations of the aphorisms of the different systems; Professor FitzEd. Hall's "Rational Refutation of the Hindu Phil. Systems," translated from the Hindee. In regard to the purely indigenous and original character of Indian philosophy, see Professor Max Müller's Appendix on Indian Logic, in Archbishop Thomson's "Laws of Thought," 3d edition. Though in the quotation given above, p. 9, Dugald Stewart speaks of the Indians having become acquainted with the Greek language and philosophy through Alexander's invasion, yet in another passage (Works, vol. i. p. 425), he thus expresses himself: "The metaphysical and ethical remains of the Indian sages are, in a peculiar degree, interesting and instructive, inasmuch as they seem to have furnished the germs of the chief systems taught in the Grecian schools." (*Diss. on the Prog. of Met., Eth., and Pol. Phil.*)

This is not to be wondered at, as the most ancient and sacred writings of the Hindus do not express themselves with any positiveness on questions of this description, but on the contrary (as we have seen in the quotations taken from some of the hymns) give frequent utterance to doubts and difficulties, as was indeed to be expected of simple men, such as their authors were, groping after truth, and unconscious of any supernatural illumination.

Without attempting to arrange these systems in chronological order, I shall give some account of their leading objects and principles.

I begin with the Pūrva Mīmānsā, which professes to supply the proper principles and rules for interpreting and applying the ceremonial portion of the Veda, but does not pretend to do more than direct the student to the attainment of those future rewards which follow the performance of duty; differing in this respect from the other five orthodox systems, which propose to themselves the higher object of pointing out the methods by which the soul may be liberated from all future connection with the body, and with mundane concerns, and may so arrive at absolute and final perfection. In pursuance of its design, the Mīmānsā defines the various modes by which duty may be ascertained and established; and as the Veda is the only ultimate authority for duty, it endeavours to show that this authority is supernatural and infallible, by maintaining, not that it has been revealed by the Deity, but that no human or personal author of it is remembered; and further, by arguing that sound is eternal, that the connection between words and the objects they denote is also eternal (and not arbitrary or conventional), and that consequently the Vedas convey unerring information in regard to unseen objects. It is curious that this system, which teaches a future state of rewards and punishments, should yet be understood by one school of its adherents in an atheistic sense. It appears from a statement made by SANKARA ACHĀRYYA, the famous commentator on the Vedānta aphorisms, that, according to the Pūrva Mīmānsā, ceremonies have an inherent power of procuring rewards; and that (whether the founder of that school acknowledged a Deity or not), he did not regard the realisation of future beatitude as at all dependent on his will.

As I have already intimated, the other systems, the Sânkhya, Yoga, Nyâya, Vaiseshika, and Uttara Mīmānsā or Vedānta, seek to accomplish a higher end than the Pūrva Mīmānsā. Though differing from each other in various other respects, they all rest upon one hypothesis, and propose to themselves one and the same general object. The supposition on which they proceed is, that the soul, after it has once become involved in a mundane condition, must go through an endless cycle of transmigrations (its happiness or misery being in each case proportioned to its merits or demerits in the preceding birth), until some means of disengaging it from all connection with the body shall have been discovered. All embodied existence is regarded by the Indian philosopher as a state of imperfection. The

happiness of paradise itself he considers (as I have already intimated) as transient, since, however long its duration, it must at last come to an end when the merit by which it has been earned is exhausted. This happiness is also essentially unsatisfactory in its nature, as the pleasure with which it is attended (even if unqualified by pain) is in itself inconsistent with the perfect tranquillity of the soul. The gods themselves are regarded as transitory beings. Thus it is said, in a verse quoted in a Sānkhya treatise :—"Many thousands of Indras, and of other deities, have passed away in every yuga (or great mundane period), for time is hard to be overcome."

The object, then, which these systems seek to effect, is to liberate the soul from every variety of embodied condition, whether earthly or celestial, so that it (the soul) may either, according to the Sānkhya and Nyāya, regain a state of perfect isolation and tranquillity, or, according to the doctrine of the Vedanta, be reabsorbed in, or rather become again conscious of its oneness with, the Divine Spirit, with which it is in reality identical. The Sānkhya, which is ascribed to the sage KAPILA as its author, acknowledges two primary principles, matter and spirit, both eternal, and consequently independent of each other. The spirit which it recognises is not, however, one supreme Soul, the existence of which it appears to deny, but a multitude of individual souls, by which the different orders of living beings are animated. Prakriti, or Pradhāna (nature, or matter),* the other principle affirmed by this system, is conceived to have originally existed in a rudimentary and imperceptible state, and, although unintelligent, to have become developed without the action of any external force, by a gradual process of spontaneous self-evolution, into all the actual forms of the phenomenal universe. The French Orientalist BURNOUF (Buddhisme Indien, 520), is of opinion that this atheistic philosophy of KAPILA was anterior to the time of 'SĀKYA MUNI (commonly

* In the text, following the example of Mr COLEBROOKE and Professor H. H. WILSON, I have spoken of *Prakriti* as *matter* (see COLEBROOKE's Essays, i. 242; WILSON's Sānkhya Kārikā, pp. 17 f., 82 f.; and also Professor FITZEDWARD HALL's translation of PANDIT NEHEMIAH GORE's "Rational Refutation of the Hindu Philosophical Systems," pp. 80, 81. But compare the preface to the same work, where the translator says, that if he had "departed from 'nature' as representing *prakriti*, he would hardly have done amiss." "'Originant' might answer, or 'evolvant.'") I find, however, that the propriety of rendering *prakriti* by "matter" is disputed. In a note which he supplied to page 221 of an article on "Indian Literature," which I furnished to the North British Review for May 1856, Professor FRASER thus expresses himself in regard to the principles of the Sānkhya philosophy:—"The Sankhya of KAPILA is the most independent and comprehensive of all the attempts of the Indian mind to form a cosmological system on grounds of reason. In its fundamental principles and method, it anticipates (after its own fashion) modern European efforts to bridge the gulf that separates the Absolute from the Relative, by means of the relation of cause and effect. The *Pracriti* thus corresponds with KAPILA to the 'Absolute' of modern speculation. In his method SPINOZA approaches KAPILA, but differs somewhat in his results. The Sankhya in many respects resembles the theory of SCHELLING, and it has some points of analogy with FICHTE. It has throughout, however, an individuality of its own, which marks its indigenous growth, and forbids us to measure all its doctrines in the forms of European speculation." It is to be remembered, however, that the "Absolute" embraces all being, whilst (as stated in the text) the Sankhya acknowledges a second primary principle, viz., *Purusha*, or Spirit, in addition to *Prakriti*.

known as Buddha), the founder of Buddhism, who is generally supposed to have flourished in the sixth century B.C.; and that the latter derived from KAPILA some of the essential principles of his own system. Though, however, it may be quite true, that 'SĀKYA derived his chief tenets from KAPILA or some earlier speculator, this is not confirmed as regards KAPILA by the aphorisms of the Sānkhya philosophy, in the state in which the collection at present exists, as this collection contains some passages in refutation of certain principles which appear to be those of different Buddhist schools. As, however, these aphorisms may be nothing more than a later summary of the original Sānkhya doctrines, the passages in question are not sufficient to prove that the author of that system lived subsequently to 'SĀKYA.

The next system, the Yoga of PATANJALI, though agreeing in other respects with the Sānkhya of KAPILA, differs from it in affirming the existence of one supreme Spirit, the governor of the universe.

The Nyāya and Vaiséshika schools are dualistic. They maintain, like the Sānkhya, the eternal pre-existence of matter, but in a different form, that of atoms, which, after being combined and developed, constitute the existing material world. These systems also assert the eternal pre-existence of a multitude of souls, distinct from each other and from the supreme Spirit, whose being the later writers, at least, of these schools distinctly recognise. The Nyāya embodies a a fully developed logical system, including a form of syllogism in five members.

The Vedanta, while recognising, in common with the other systems, the dogma of transmigration, has a widely different theory in regard to matter and spirit. Its essential principle is *adwaita*, non-duality, viz., that there is only one substance, or principle of being. This is Brahma, the universal Spirit. The souls by which living beings are animated, though apparently individual and distinct, both from each other and from the Supreme Spirit, are in reality one with the latter. They are not even emanations or portions of it; they are identical with it, and are only withheld from a consciousness of this identity by the illusion of ignorance, arising from the mundane condition in which they have become involved by the will of the Supreme Spirit, or by the eternal revolution of cause and effect. But it is not merely the identity of all spirit that the Vedanta maintains; it also asserts, according to the older form of the doctrine, as explained by Mr COLEBROOKE, that the phenomenal world is evolved out of the substance of Brahma. According to the more modern theory, however, the external world has no real, but merely an apparent or illusory, existence. It is not necessary that I should here attempt to expound all the refined details of this subtle and elaborate system.* The above rough outline must suffice.

* From the account I have given of this philosophy in the article in the "North British Review," above referred to, I extract the following passage:—"The doctrine accordingly takes a somewhat different form in its gradual development. Assuming the same essential principle of non-duality—

The Vedanta is regarded as the most conformable to the Veda, and consequently as the most orthodox, of all the five systems which I have described. There can be no doubt that these schools are, in many respects, antagonistic to one another; and that in ancient times the founders and adherents of the other systems maintained as exclusively true the principles which their own speculation had led them to embrace, without any deference to the Vedanta, as more authoritative. All the systems profess to be founded on, or at least consistent with, the Veda, the supreme authority of which, as I have already stated, they recognise and enforce by different arguments. It is evident, however, that some of them, such as the Sānkhya and Nyāya, are founded far more on speculation than on any data supplied by the Veda; and that though their adherents could no doubt find some passages of the latter which support their doctrine, the Vedanta (while it has to explain away particular texts), is in general more conformable to the spirit of the later portions of the Hindu Scriptures,—I mean the Upanishads. I say the "later portions," for the Vedanta (in common with all the other schools, excepting only the Pūrva Mīmānsā) attaches very little importance to the earlier and greater division of the Vedas,—including nearly the whole of the hymns, and the ceremonial part of the Brāhmanas, which, in its idea, do nothing more than prepare the way for the knowledge of Brahma, the only object of study which is of any real consequence. In assigning this subordinate rank to the mythological and ritual portion of the Veda, the Vedanta only follows what we have already seen to be the doctrine of the Upanishads themselves.

Although, however, it is clear that in ancient times the Vedanta did not enjoy any greater authority than the other schools, it has now come to be recognised as the highest expression of Indian orthodoxy, and the study of the other systems is regarded as merely preparing the way for its full comprehension. This is distinctly affirmed in the Prasthāna-bheda, a native summary of Indian literature by MADHUSUDANA SARASVATĪ, which at the same time defines as follows the leading tenets of the several systems:—

"The difference in principle between these various schools is, when briefly stated, threefold. The first doctrine is that of a *commencement* of the world; the second is that of an *evolution* (or, *modifica-*

viz., that nothing exists but Brahma, or Deity—it no longer holds that he is transubstantiated into the material world, for the objects of sense, it is maintained, have no real existence at all. The outward world is only an illusion, just as when a rope (to use the Indian illustration), lying on the ground, is by mistake taken for a serpent; but this does not imply that the rope has been changed, or has really become a serpent. In the same way, when it is said that the universe is Brahma, it is not meant that he has really become the universe, but that he appears so. The existence which the world appears to have is no real existence of its own, but the existence of Brahma attributed to it. The name and the form which it has are derived from *māyā*, or illusion. Brahma is the substratum of the illusion; that is, he is not really the material cause of the world, as earth is of a jar, but he is such a substratum as the rope is of the serpent, for which it is mistaken. As the fancied existence of the serpent depends on the real existence of the rope, while the latter is not actually changed into a serpent, so, too, the seeming existence of the finite universe depends on the real existence of Brahma, though Brahma is not actually changed into the universe."

tion); the third is that of an *illusion*. The first theory, that of the Logicians and Mīmānsakas is this: atoms of four descriptions—earthy, aqueous, igneous, and atmospheric—beginning with compounds of two atoms, and ending in the egg of the Brahmā (the world), originate the universe; and effects *previously non-existent come into being* from the action of a causer. The second theory, that of the Sānkhyas, Pātanjalas, and Pāsupatas, is that *Pradhāna* (or *Prakriti*, nature), consisting of the three *guṇas* (qualities), *sattva*, *rajas*, and *tamas* (goodness, passion, and darkness) is *evolved*, through the successive stages of *mahat* (intellect), and *ahankāra* (consciousness), &c., in the shape of the world; and that effects which had previously existed in a subtile form are (merely) *manifested* by the action of a cause. Another form of the theory of *evolution* is that of the Vaishṇavas, who hold the world to be a *development* of Brahma. The third view, that of the Brahma-vādīs (*i.e.*, the Vedantists), is that Brahma, the self-resplendent, the supremely happy, and the one sole essence, assumes, unreally, the form of the world through the influence of his own illusion (*Māyā*)."

Having thus explained the principles of the different systems, the author proceeds,—

"The ultimate scope of all the sages (*munis*), the authors of these different systems, is to support the theory of illusion, and so to establish the existence of one God, the sole essence; for these sages could not have been mistaken [as some of them must have been, if they were not all, of one opinion], since they were omniscient. But as [they saw that] men, addicted to the pursuit of external objects, could not all at once penetrate into the highest truth, they held out to them a variety of theories, in order that they might not fall into atheism. Misunderstanding the object which these sages thus had in view, and representing that they even designed to propound theories contrary to the Veda, men have come to regard the specific doctrines of these several schools with preference, and thus become adherents of a variety of systems."

So far MADHUSŪDANA. The vast amount of thought which has been expended upon these philosophical systems is evinced by the large number of commentaries and treatises which have been composed for their illustration. It has frequently occurred that the most celebrated commentaries have themselves in turn formed the subject of glosses by later writers. The effect of this unceasing and long-continued speculation has been, as might have been anticipated, that the doctrine of the several schools has in process of time undergone, in some respects, considerable modifications, and that some of the schools have become subdivided. But the activity of the Hindu mind in philosophical thinking is not to be judged by a reference to these six so-called orthodox systems only. There are many others, such as those of the Rāmānujas, Madhwāchāryyas, Pāsupatas, which are regarded as less authoritative, and some which are characterised as absolutely heretical,—such as those of the different Buddhist schools, and the Chārvākas, or Materialists, &c. &c.

Besides these systems of philosophical speculation, there is a large body of literature of other kinds which has grown out of the desire to understand the Veda aright, and apply its texts and precepts correctly to the various exigencies of religious worship and of social life. This literature is technically divided into six Vedāngas, or members of the Veda,—viz., the sciences of pronunciation, religious ceremonial, grammar, prosody, astronomy, and interpretation. The rules of pronunciation are minutely explained in the ancient treatises called Prātisākhyas, of which several are extant. The rules of religious worship, as well as prescriptions for the regulation of domestic and social life, are contained in certain

other sets of aphorisms, in which the results of older works and traditions are reduced into a condensed and more scientific form. These aphorisms, in their turn, have supplied the materials for the formation of such later works as the ceremonial and legal institutes of Manu, Yājnavalkya, &c. The department of grammar is now represented by a very large literature. This science has been carefully studied in India from an early period. The famous grammarian PĀṆINI, the oldest whose complete works have come down to us, is considered to have lived several centuries before the Christian era;* but he alludes to various authors, and even to two schools of grammarians, as having preceded him. YĀSKA, too, the interpreter of the Veda, who is regarded as older than PĀṆINI, refers by name to numerous grammarians and etymologists who lived before his time. The eight books of PĀṆINI contain, in the form of short aphorisms, a complete grammatical system, founded no doubt on the labours of his predecessors, as well as on his own observations. The aphorisms are not only brief, but very technical. For instance, the various affixes by which the formation of derivatives and the inflection of words and roots are effected, are denoted by letters which, being rather algebraical symbols than distinct representations of the changes which their application is intended to produce, could not be understood without accompanying explanations. These aphorisms have become the subject of minute annotation by two very ancient authors, KĀTYĀYANA and PATANJALI (whose Mahâbhâshya is an elaborate work of great extent), and by others; and have also formed the basis of numerous grammars in which the materials have been differently arranged, as well as the occasion of other treatises devoted to the discussion of particular topics. The Prākrit (or mediæval vernacular) languages have also been treated in different grammatical works, which contain the rules according to which these different dialects modify the words and inflections of the Sanskrit.

It is now time that I should say something of the remarkable religious revolution of which India had in the meantime become the theatre. 'Sākya Muni, or Gautama Buddha, the author of this great movement, belonged to the warrior caste, and was the son of an Indian prince. Being of a thoughtful and meditative turn of mind, he abandoned his father's court, betook himself to a religious life, and studied under the most famous Brahmanical teachers of his day. He was not, however, satisfied with their doctrines. Though he held fast by the old Indian tenet of the transmigration of souls, he founded on it a new superstructure of his own, in many respects different from that of the Brahmans. The substance of the new system was, that by the attainment of transcendental knowledge, and by the enlightened practice of moral virtues, men could be delivered from the miseries of renewed mundane existence, and enter into the condition of *nirvâna*. This doctrine he preached to people of all classes without distinction, and in the vernacular tongue. In both of these

* See Professor MÜLLER's Anc. Sansk. Lit., pp. 304, ff.; Professor GOLDSTÜCKER's "Panini, his place in Sanskrit Literature."

respects Buddha departed from the principles of the Brahmans, who originally held that the highest knowledge was, primarily, at least, the exclusive privilege of the superior castes, and who communicated that knowledge through the medium of learned treatises in an abstract and scholastic shape. The new teacher rested his pretensions, first, on the benevolence of his character and the sanctity of his life; and, secondly, on the supernatural knowledge and faculties which were regarded as necessarily springing from that sanctity. His own past existence had not been exclusively divine; he had passed through various stages of transmigration, gradually accumulating merit, and earning the favour of the preceding *Buddhas;* and by their grace, and his own efforts, he was now advancing to the highest perfection. This system recognised no supreme Deity. The previous Buddhas had risen to their divine dignity by the same stages as their successors were to pass through. As Buddha prohibited the destruction of animal life, and was therefore opposed to the sacrificial rites of the Veda, and as his doctrine evidently tended to set aside the prerogatives of caste, he was certain to encounter the opposition of the Brahmans. The Buddhistic legends recount the various displays of miraculous power by which he discomfited his opponents, and persuaded multitudes to embrace his doctrines. The new creed triumphed over Brahmanical opposition, gained an increasing number of adherents, and a few centuries after the founder's death was not only widely diffused over Northern India, but had also been propagated by zealous missionaries in the surrounding regions. It is, however, a remarkable fact, that this system, which retained such a permanent hold on foreign countries such as Ceylon, Burma, and China, was destined to die out on the soil from which it had sprung. The causes of this decline and extinction of Buddhism in India have not yet been properly investigated. It is greatly to be lamented that the illustrious scholar, EUGÈNE BURNOUF, who threw so much light on the origin of Buddhism,* should not have lived to extend his researches into the later fortunes of this religion in its native country. Whether it was that the Brahmans, by regaining their ascendant over the minds of the Indian princes, or by accommodating their mythology and ceremonial to the popular taste, succeeded in overpowering the Buddhists, and in expelling them from Hindostan, or in terrifying them into submission, or whether it was that Buddhism, by abandoning its original principles, had lost its early influence, and so merged into the superstitions by which it was surrounded,—the fact is undoubted, that it has now disappeared from every part of the Indian continent except Nepal. As *nirvāna*, the final goal to which Buddhism professes to conduct its disciples, is considered by most scholars to mean nothing different or distinguishable from annihilation, the wonder is rather that this system should have continued to flourish anywhere, than that it should have ceased to exist in India.

* See his "Buddhisme Indien," from which the above sketch has been derived, either immediately or at second hand; M. BARTHELEMY ST HILAIRE's work on the same subject; Professor H. H. WILSON's paper on Buddhism, in the Journal of the Royal Asiatic Society for 1856; and Professor MAX MÜLLER's Sketch of Buddha's career.

The limits within which I am necessarily restricted will not allow me to supply any further details regarding the subsequent religious history of India, or regarding the Itihāsas (the so-called epic poems), and the Puranas, those voluminous collections of legends and mythological stories, in both of which classes of works that religious history may be obscurely traced. Nor can I give any particular account of the later poetry, narrative and dramatic, of the Hindus, which, by its polished, elaborate, and rhetorical character, is widely distinguished both from the simplicity of the Vedic hymns, and from the tiresome and inartificial prolixity of the epic poems and most of the purānas. I must also omit everything but a passing reference to the treatises on algebra and astronomy by which the Indians have established their title to an honourable rank in science as well as in literature. I will only mention, that in these treatises they repudiate the mythological fiction of the earth being supported on a serpent or a tortoise, and assert its rotundity and self-sustaining position in space; and that ĀRYA BHATTA, one of their most ancient astronomers, maintained the rotation of our globe on its own axis.

Enough has however been already said to refute the old idea that the Hindus are a stationary and unchangeable people, to give some conception of the great antiquity of their civilisation, of the chief productions of their genius, and of its wide range and versatility, and to show that, though inferior to the Greeks in correctness of taste, in the sense of harmony and proportion, in robustness of intellect, and in practical sagacity, they yet rival their great Western kinsmen in speculative subtlety, originality and elevation.*

* I quote from the "North British Review" for May 1856, p. 213, the following estimate of the Indian intellect:—"The Indo-Arians partake largely in all the higher qualities of the Indo-Germanic race—in their capacity of self-development, their intellectual power, their love of science, their tendency to metaphysical speculation, their aspirations after ideal and spiritual perfection, their taste for the fine arts and for elegant literature. And yet they are distinguished by marked characteristics of their own, corresponding to their position as an Asiatic nation. They do not possess the masculine nature of the kindred tribes who migrated to the north-west. While the Greeks, with all their speculative genius and exquisite sense of the beautiful, were a restlessly active, energetic, and practical people, the Hindus, on the other hand, have manifested a strong tendency to repose, and to dreamy contemplation. While the Greeks sought, as far as possible, to realise their conceptions of ideal truth and good in the outward world, in forms of visible beauty, or of political organisation, the Hindus, rejecting the material universe as a theatre or permanent instrument of perfection, came soon to regard the world of the senses as, on the contrary, the necessary source of all evil and disorder, and to seek their chief good in a purely spiritual state, emancipated from all mundane relations, and from ordinary human feelings and interests. Their philosophy, properly so called, has all a religious aim; every branch of it professes to unfold a scheme of knowledge of which the declared end is to enable its possessor to free himself from the bondage of worldly existence. Its logical and metaphysical systems, while displaying wonderful acuteness and subtlety, are too much concerned with abstruse and unpractical niceties, and with the controversial anticipation of all possible and impossible objections. Gifted with a luxuriant imagination, with tenderness of feeling, with sensibility to natural impressions, with a delicate perception of the nicest shades of thought, and of the harmonies of language, the Hindus are yet deficient in correct taste, and in a sense of the true sublime: their poetical power is wasted on tasteless refinements or jingling alliterations; and when dealing with the vast, or the terrible, they are prone to mistake exaggeration and aggregation of magnitude and numbers for forcible and impressive representation." See LASSEN'S great work on Indian history, the "Indische Alterthumskunde," vol. i. pp. 412, ff.

XXIV.—*On Fagnani's Theorem.* By H. F. Talbot.

(Read 20th April 1863.)

Before proceeding to the subject of this paper, I wish to advert to the following well-known theorem.

Let ABC be any triangle with its inscribed and circumscribed circles. Then if we take any other point D in the exterior circle, and draw the tangents DE, EF, FD, the last tangent will come again to the original point D. A similar theorem is true of a polygon of any number of sides which is both inscribed in, and circumscribed to, a circle. Also if ellipses are substituted for circles. Of these theorems I remember to have seen a demonstration founded on the theory of elliptic integrals. But this appears to me to be a great waste of analytic power; for the theorem really results from first principles, as I think will be manifest from the following considerations.

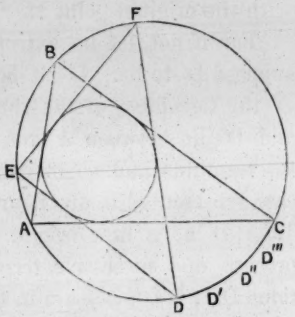


Fig. 1.

I shall take the simplest case, that of two circles inscribed in, and circumscribing, a triangle, because the reasoning is similar in the more complicated cases.

Lemma 1. If there are two circles, one within the other, and if AB, CD, are two chords of the outer circle, touching the inner circle, then if C lies between A and B, D does not.

For it is plain that a line drawn from C to any other point in the arc AB, would not touch the inner circle, which is contrary to the hypothesis.

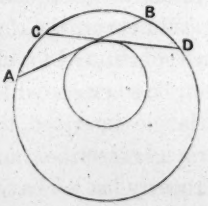


Fig. 2.

If a circle lies anyhow within another, and tangents 1, 2—2, 3—3, 4, be drawn, then, generally speaking, the points 1 and 4 do not coincide. The arc 1, 4, may be called the *deviation*, and if more tangents 4, 5—5, 6—6, 7, be drawn, the arc 4, 7, is the *second deviation*.

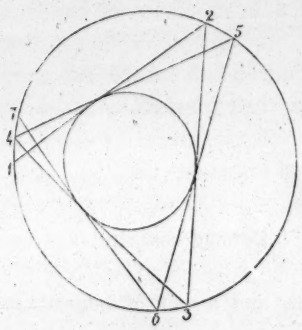


Fig. 3.

Lemma 2. The successive deviations are constantly *in the same direction*, and consequently the total deviation increases till at length it surpasses any given arc of the circle.

Demonstration. Since the point 4 (as represented in fig. 3) lies between 1 and 2, the point 5, at the other end of the tangent, lies between 2 and 3 (by the first lemma), and for the same reason the point 6 lies between 3 and 4.

Consequently, 7 lies between 4 and 5; that is, the deviations 1, 4, and 4, 7, are in the same direction. And the total deviation tends to increase without limit, because it is plain, from the nature of the problem, that the deviations are quantities of the same order (though sometimes larger, sometimes smaller.)

Theorem I. (see fig. 1.) If a triangle ABC is inscribed in one circle and circumscribed about another, then if any point D be taken in the outer circle, and three tangents, DE, EF, FD, be drawn, the extremity of the last will coincide with the original point D.

For if not, let its extremity be D′. It is immaterial on which side of D we suppose D′ to lie. Let it be between D and C. Then calling D the origin, and D′ the terminus, of the three tangents, it follows from lemma 1, that both D and D′ lie between A and C. For however near to C we take an origin, the terminus and origin cannot lie on *opposite* sides of C. Now to return to the case represented in our figure. We have supposed D the origin, D′ the terminus. Take D′ as a new origin, and D″ will be the new terminus. Continue the process, and successive termini D‴, D⁗, &c., will be obtained. All the deviations DD′, D′D″, &c., are in the same direction, and the total deviation ultimately surpasses any given arc, and therefore surpasses the arc DC (by lemma 2). But this result involves an absurdity, since it implies that a certain origin D_n and terminus D_{n+1} are on opposite sides of C. Therefore the supposition from which we started, viz., that D and D′ are different points, leads to a false conclusion. Consequently those points are the same. *Q.E.D.*

Moreover, the theorem is true when two ellipses, one lying within the other, are substituted for circles. For, by SALMON's Conic Sections, p. 308, two ellipses can be so projected that both will be circles, and of course the projection of the triangle inscribed to the one and circumscribed to the other ellipse, will be a triangle inscribed and circumscribed to the two circles. Such a triangle has the property (as we have just shown) of remaining always *closed* or *complete*, however we vary the point of origin, which may be taken in any part of the outer circle.

And therefore all triangles inscribed and circumscribed to the two given ellipses are closed and complete. For otherwise their *projections* could not be so.

Theorem on the Ellipse.

In SALMON's Conic Sections, p. 297, we find the following beautiful theorem, due to Dr GRAVES.

If two ellipses have the same centre and focus, and a pair of tangents be drawn from any point of the outer to the inner ellipse, the difference between the sum of the tangents and the intercepted elliptic arc is a constant quantity.

From this theorem a very remarkable consequence may be drawn. Let ABC, DEF, be the two ellipses having same centre O and foci S, H. Moreover, let the axis major of one bear such a proportion to that of the other, that if two chords, AB, BC, be drawn in the outer ellipse touching the inner one at E and F, the third chord CA shall also touch it at D. Then by GRAVES'S theorem BE+BF−arc EF=C, a constant. Also CF+CD−arc FD=C, and AD+AE−arc DE=C. Therefore adding these equations together, we find that the periphery of the triangle ABC, *minus* the periphery of the ellipse =3 C. Now, by the preceding theorem I., we know that if, instead of A, we take any other point A′ of the outer ellipse as the origin, and draw tangents A′B′, B′C′, C′A′, from it to the inner ellipse, they will form a closed triangle. Its periphery, minus that of the ellipse, will =3 C, by the same reasoning as before. Therefore the peripheries of the two triangles are equal. But the same reasoning applies to a closed polygon of any number of sides. Whence the following theorem:—"If two ellipses, with same centre and foci are such that a polygon can be inscribed to the one and circumscribed to the other, the periphery of the polygon is constant, whatever point of the outer ellipse be assumed as its origin."*

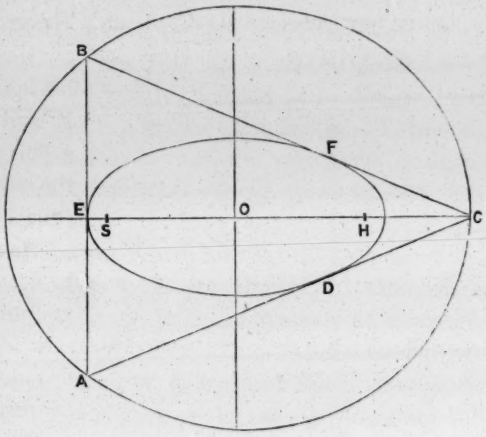


Fig. 4.

After I had arrived at this theorem, I found that M. CHASLES had given one nearly resembling it (Comptes Rendus, 1843, p. 838), with the additional remark, that since every two consecutive sides of the polygon are equally inclined to the periphery of the outer ellipse, a ray of light might describe such a polygon, by successive reflections at the periphery; and therefore it would always be a tangent to an inner confocal ellipse. But I think this remark may be rendered yet more striking by considering the cases in which the polygon is not closed. If this occurs when the origin is any point A, it will also occur when any other point is assumed as the origin. The path of the ray of light under these circumstances would be constantly varying, but always tangent to the inner ellipse, so that ultimately, after an infinite number of reflexions, the ray will have passed

* It is unnecessary to say with CHASLES that they must be polygons of the same number of sides, for they cannot be otherwise. M. CHASLES, in his paper here referred to, gives *no demonstrations*. I am not aware whether they were subsequently published.

over every part of the space intercepted between the ellipses, leaving untouched the whole of the interior ellipse.

Fagnani's Theorem.

Let us now consider MacCULLAGH's Theorem, given by SALMON, p. 297.

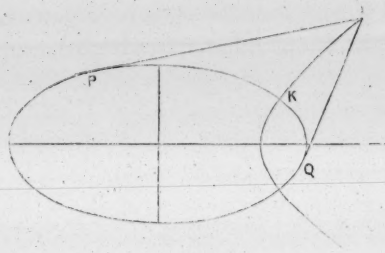


Fig. 5.

If any ellipse is cut by a hyperbola, having same centre and focus in the point K, and from any point T of the hyperbola a pair of tangents TP, TQ, is drawn to the ellipse, the difference of the tangents equals the difference of the arcs PK, QK.

As an example of this, let tangents be drawn at the extremities of the major and minor axes, meeting in T, and let a hyperbola, having same centre and focus, be so drawn as to pass through T, and cut the ellipse in K. Then the elliptic quadrant PQ is so divided in K, that $PK-KQ=PT-TQ=a-b$, or the difference of the semiaxes of the ellipse, which is FAGNANI's theorem (SALMON, p. 298).

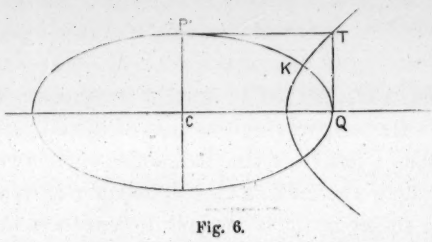


Fig. 6.

Now, let the point T advance continually along the hyperbola, and the intercepted arc will of course always be divided at the same point K. Ultimately, the point T may be supposed to attain the asymptote at an infinite distance. The two tangents TP, TQ, are then parallel to the asymptote. Their

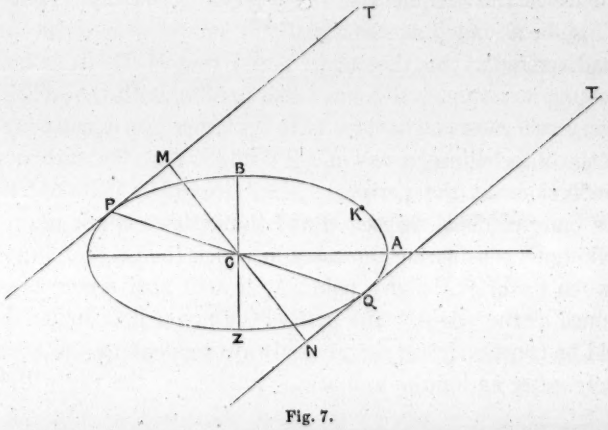


Fig. 7.

difference is equal to the difference of their extremities which touch the ellipse, and are cut off by any line at right angles to them. Call this difference D. The intercepted arc is then a semi-ellipse, because, when tangents are parallel, the line joining the points of contact passes through the centre.

Now we have $PK-QK=PT-QT=D$, and $BK-AK=a-b$; therefore, by subtraction, $PB-QA=D-(a-b)$. But the arc $PB=QZ$, $\therefore$ $QZ-QA=D-(a-b)$. But ZA is a quadrant of the ellipse, therefore this quadrant is so divided in the point Q, that the difference of QZ and QA is a known straight line $=D-(a-b)$.

Now, it is by no means obvious, whether or not we have thus obtained a division of the elliptic quadrant different from the one we first obtained. This point can only be settled by a rigorous demonstration; the result of which gives this curious theorem $D=2(a-b)$. From which we see that $QZ-QA=a-b=BK-KA$, so that the elliptic quadrants BA and ZA are divided at corresponding points K and Q, and the arc $AK=AQ$. And since the tangent at Q or QT is parallel to the asymptote, it follows, by parity of reasoning, that the tangent at K is parallel to the other asymptote. Moreover, if we draw MCN through the centre, at right angles to PT and QT, it is plain that $D=PM+QN$, or since these lines are equal, $2(a-b)=D=2QN$ $\therefore$ $QN=a-b$, hence the point of division Q is such, that the perpendicular let fall from the centre on the tangent at Q, cuts off from it a portion $QN=a-b$.

It remains, therefore, to demonstrate *à priori* the theorem we have just indicated, viz., that $QN=a-b$, whence the other properties mentioned will follow. We shall, at the same time, obtain the demonstration of many other theorems.

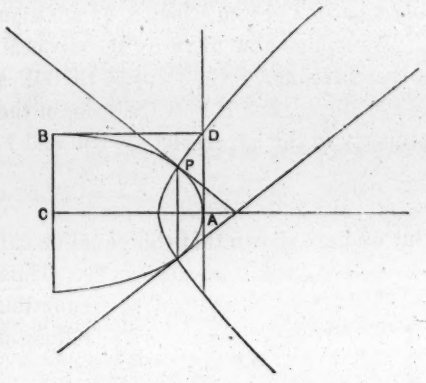


Fig. 8.

Let CA, CB, be the semi-axes of an ellipse, denoted by a, b. Complete the rectangle BCAD. Let A, B, be the semi-axes of a hyperbola, having same centre and focus, and so drawn as to pass through the point D, whose co-ordinates are a, b. Then, according to SALMON (Conic Sections, p. 298), the co-ordinates of the point P, which is the intersection of the two curves, are, $x^2=\frac{a^3}{a+b}$, $y^2=\frac{b^3}{a+b}$.

We shall reverse this order of reasoning, and suppose a hyperbola drawn with centre C, axis in the line CA, and passing through the points D and P, and then show that such a hyperbola has the same focus with the ellipse. In the first place, then, the point whose co-ordinates are $x^2=\frac{a^3}{a+b}$, $y^2=\frac{b^3}{a+b}$ lies in the ellipse,

for those co-ordinates verify the equation to the ellipse $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$. The substitution gives $\frac{a}{a+b}+\frac{b}{a+b}=1$, which is identically true.

Secondly. By hypothesis, the equation of the hyperbola $\frac{x^2}{A^2}-\frac{y^2}{B^2}=1$, is satisfied by the values $x=a$, $y=b$, which gives (1) $\frac{a^2}{A^2}-\frac{b^2}{B^2}=1$, and also by the other values $x^2=\frac{a^3}{a+b}$, $y^2=\frac{b^3}{a+b}$.

which gives (2) $$\frac{a^3}{(a+b)A^2}-\frac{b^3}{(a+b)B^2}=1.$$

Subtracting (2) from (1) $$\frac{a^2b}{(a+b)A^2}-\frac{b^2a}{(a+b)B^2}=0$$

$\therefore \frac{a}{A^2}=\frac{b}{B^2}$ which gives the remarkable result $A^2 : B^2 :: a : b$, showing that the elliptic semi-axes are in the duplicate ratio of the hyperbolic ones. Hence if $A^2=ka$, $B^2=kb$ (k being an indeterminate.)

To determine its value, we resume the equation (1), $\frac{a^2}{A^2}-\frac{b^2}{B^2}=1$

which gives $\frac{a^2}{ka}-\frac{b^2}{kb}=1$, whence $k=a-b$.

Therefore the squares of the semi-axes of the hyperbola, are $A^2=a\ (a-b)$, $B^2=b\ (a-b)$. From whence, by addition, $A^2+B^2=a^2-b^2$.

It remains now to verify the confocality of the two curves. If C is their common centre, and F the focus of the ellipse, we have $CF^2=a^2-b^2$, by the property of the ellipse, and if F′ is the focus of the hyperbola, we have $CF'^2=A^2+B^2$ by the property of the hyperbola. $\therefore$ if F and F′ are the same point

$$CF^2=CF'^2 \text{ or } a^2-b^2=A^2+B^2.$$

But we have shown that this equation exists, and therefore the curves are confocal.

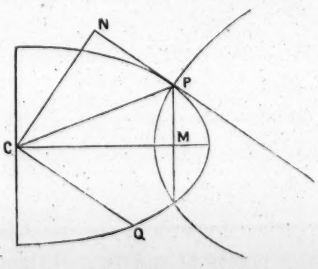


Fig. 9.

Thus we have proved that the squares of the co-ordinates of the point P, where the confocal ellipse and hyperbola intersect, are,—

$$x^2=\frac{a^3}{a+b},\ y^2=\frac{b^3}{a+b}.$$

Therefore (1) $CP^2=x^2+y^2=\frac{a^3+b^3}{a+b}=a^2-ab+b^2$

Let CQ be the conjugate to CP.

$\therefore$ (2) $CP^2+CQ^2=a^2+b^2$ by a property of the ellipse.

Subtracting (1) from (2), we find $CQ^2=ab$.

Let CN be perpendicular on the tangent PN, then by another property of the

ellipse, $CN \cdot CQ = ab$; whence $CN^2 \cdot CQ^2 = a^2b^2$. Divide this equation by $CQ^2 = ab$ $\therefore CN^2 = ab$, and $\therefore CN = CQ$. But now, since $CP^2 = a^2 + b^2 - ab$, and $CN^2 = ab$, therefore $PN^2 = CP^2 - CN^2 = a^2 + b^2 - 2ab$, $\therefore PN = a - b$, which is the theorem we undertook to demonstrate.*

It is curious that CQ when prolonged, becomes the asymptote of the hyperbola. Perhaps we have already offered sufficient proof of this, but the reader may not object to see it proved in another manner.

The equation to the ellipse $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$ gives $\frac{dy}{dx} = -\frac{x}{y} \cdot \frac{b^2}{a^2}$. But the value of $\frac{x}{y}$ at FAGNANI'S point is $\frac{a^2}{b^2} = \frac{A^2}{B^2}$. Therefore $\frac{dy}{dx} = -\sqrt{\frac{b}{a}} = -\frac{B}{A}$. But by the property of the hyperbola, if C be the centre and CA, CB (or A, B), the semi-axes, the asymptote CD will be found by completing the rectangle ACBD, and joining CD. $\therefore$ the asymptote makes, with the axis CA an angle, whose tangent $= \frac{DA}{CA} = \frac{B}{A}$, and the other asymptote makes an equal angle below the axis, whose tangent is therefore $-\frac{B}{A}$. But we found $\frac{dy}{dx} = -\frac{B}{A}$; therefore, the tangent to the ellipse at the point P, or (x, y) is parallel to the *second* asymptote. Consequently, the conjugate semi-diameter CQ, is a portion of that asymptote.

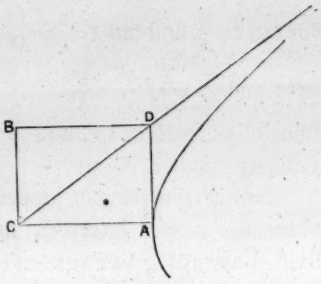


Fig. 10.

Since the two curves are confocal, they ought to intersect at right angles.

Let us verify this. We have seen that the equation to the ellipse gives $\frac{dy}{dx} = -\frac{B}{A}$ at FAGNANI'S point. But the equation to the hyperbola $\frac{x^2}{A^2} - \frac{y^2}{B^2} = 1$ gives at the same point $\frac{dy}{dx} = \frac{x}{y} \cdot \frac{B^2}{A^2} = \frac{A^3}{B^3} \cdot \frac{B^2}{A^2} = \frac{A}{B}$. But these two results, $\frac{B}{A}$ in the ellipse, and $\frac{A}{B}$ in the hyperbola (neglecting the signs) are reciprocals. Therefore at the point of intersection the two curves make angles with the axis, whose tangents are reciprocals, and therefore they intersect at right angles.

Hence this curious theorem "At the point of intersection P, the normal to the hyperbola is parallel to one of its asymptotes."

* These properties, viz., that $CN = CQ = \sqrt{ab}$, and that $PN = a - b$ are proved by BRINKLEY in quite a different manner (Trans. of the Royal Irish Academy, vol. ix. p. 146, &c.). He likewise proves, that if CQ produced cuts in O, the circle described on the axis major as diameter, a perpendicular let fall from O on the axis, cuts the ellipse in FAGNANI'S point. But I have shown that CQ produced is the asymptote of the hyperbola, $\therefore$ an ordinate to the ellipse at FAGNANI'S point, passes through the intersection of the asymptote and circle. In other words, the common chord of the ellipse and hyperbola, being produced, becomes the common chord of the circle and the two asymptotes.

For that normal is tangent to the ellipse, and the asymptote coincides with the conjugate diameter.

Now, let us draw three vectors to the hyperbola, from the centre, making the angles θ, θ', θ'', respectively with the axis. The first to be drawn to a point infinitely distant (and, therefore, it will be the asymptote), the second to the point D, and the third to the point P (see fig. 8). Then, if we call $\tan\theta = t$, we have the following curious property,—

$$\tan\theta' = t^2$$
$$\tan\theta'' = t^3$$

For $\tan\theta = \frac{B}{A}$, and $\tan\theta' = \frac{b}{a} = \frac{B^2}{A^2}$ and $\tan\theta'' = \frac{y}{x}$, at the point where the curves intersect; and $\therefore = \sqrt{\frac{b^3}{a^3}} = \frac{B^3}{A^3}$.

Since θ is less than 45°, and $\therefore$ $\tan\theta$ less than 1, the angles θ, θ', θ'', become successively smaller.

Another remarkable property is the following (see fig. 9):—At the point P, where the curves intersect, and where the elliptic quadrant is algebraically divided according to FAGNANI's theorem, the line PN intercepted by the perpendicular CN on the tangent, is a maximum. For if we examine in *any* ellipse what must be the position of the point P, in order that PN may be a maximum, we shall find it to be characterized by those values which we have already shown to belong to P in FAGNANI's theorem. This may be shown as follows:—

In any ellipse, let C be the centre, CP, CQ, conjugate diameters, PN a tangent, and CN or p the perpendicular to it.

We have $$CQ = \frac{ab}{p} \quad \therefore \quad CQ^2 = \frac{a^2b^2}{p^2}.$$

But $$CP^2 = a^2 + b^2 - CQ^2 = a^2 + b^2 - \frac{a^2b^2}{p^2}.$$

Subtract $$CN^2 = p^2$$

$$\therefore PN^2 = a^2 + b^2 - \frac{a^2b^2}{p^2} - p^2.$$

Let this be a maximum. Therefore since $a^2 + b^2$ is constant, $\frac{a^2b^2}{p^2} + p^2$, must be a minimum, or putting $a^2b^2 = n$, and $p^2 = x$, then $\frac{n}{x} + x$, is a minimum.

Differentiating this, we find $x = \sqrt{n}$; or $p^2 = ab$. From this value we find $CQ^2 = ab$, and $\therefore$ $CQ = p$, and $PN^2 = a^2 + b^2 - ab - ab$; $\therefore$ $PN = a - b$. And as these were the values which we found before for the same lines, it is evident that P is the same point which we were considering before. Therefore, at the point P, which may be called FAGNANI's point, the line PN is a maximum.

Additional Remarks.

I will here add two or three other theorems, which have suggested themselves in the course of this inquiry.

If BCA is an ellipse, and P is FAGNANI's point, and the tangent OPT is drawn terminated by the axes produced, then OP=CA, and PT=CB.

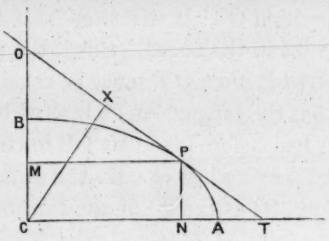


Fig. 11.

Demonstration.—Let CA=a, CB=b, CN=x, PN=y. The equation is $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$. At FAGNANI's point we have,

$$x^2=\frac{a^3}{a+b} \text{ and } y^2=\frac{b^3}{a+b}.$$

Therefore $a+b=\frac{a^3}{x^2}=\frac{b^3}{y^2}$. Hence $a(a+b)=\frac{a^4}{x^2}$ and $b(a+b)=\frac{b^4}{y^2}$.

Whence by addition $(a+b)^2=\frac{a^4}{x^2}+\frac{b^4}{y^2}$.

Now we have by a general property of the ellipse $CT=\frac{a^2}{x}$ and $OC=\frac{b^2}{y}$, whence $OT^2=\frac{a^4}{x^2}+\frac{b^4}{y^2}$, which we have just proved to be equal to $(a+b)^2$. Therefore we have the curious result, OT=$a+b$.

Now, $$OT : OP :: CT : MP;$$

or, $$a+b : OP :: \frac{a^2}{x} : x.$$

Therefore $OP=(a+b)\frac{x^2}{a^2}$. And since at FAGNANI's point $\frac{x^2}{a^2}=\frac{a}{a+b}$, it follows that OP=$a$. And similarly it is shown that PT=b.

From whence the following curious theorem follows,—It is well known that if OCT is a right angle, and OT is a line of given length, which moves so as to keep its extremities constantly in the lines CO, CT, then any point P of the line OT will generate an ellipse. But there is only one position of the line OT in which it touches the ellipse.

Theorem.—If the generating line OT touches the ellipse, the point of contact P is FAGNANI's point.

Hence, if we let fall the perpendicular CX upon OT, then OX=PT=semi-axis minor. For OP=a, and we have seen in the course of this investigation, that

$$PX=a-b \therefore OX=OP-PX=b.$$

The following corollary is also worth remarking.

$$OP-\text{arc } BP=PT-\text{arc } PA.$$

And also the following:—Of all the lines, which touch the ellipse, and are terminated by the axes produced, the shortest is OT, which touches at FAGNANI'S point.

The simplest proof of this, is by the doctrine of infinitesimals. Let the line of constant length OT (which we called above the generating line) assume another position O′T′, it will now be a secant to the ellipse, and P will occupy another point in the curve. But if the position O′T′ be taken infinitely near to OT, and P′ to P, then O′T′ must be considered as still being a tangent; and thus we see that the tangent at P′, limited by the axes produced, has the same length which it had before [the part OP having gained an infinitesimal quantity δ, and the part PT having lost exactly the same], which is the character of a minimum. Therefore, OT is the minimum tangent, terminated by the axes produced. It is curious, that while $OT=a+b$ is the *Minimum* of its kind, $PX=a-b$ is the *Maximum* of its kind, as we proved in another part of this Memoir.

This result may also be obtained by the differential calculus, as thus:—

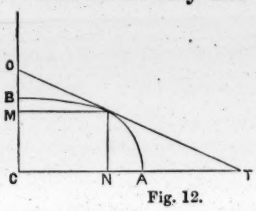


Fig. 12.

$$CT=\frac{CA^2}{CN}=\frac{a^2}{x}$$

$$OC=\frac{CB^2}{CM}=\frac{b^2}{y}$$

$$\therefore OT^2=\frac{a^4}{x^2}+\frac{b^4}{y^2}$$

And when OT is a minimum,

$$\frac{a^4dx}{x^3}=-\frac{b^4dy}{y^3}.$$

But by differentiating the equation to the curve, $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$, we find $\frac{xdx}{a^2}=-\frac{ydy}{b^2}$, which is always true.

Dividing the first of these equations by the second, we get $\frac{a^6}{x^4}=\frac{b^6}{y^4}$ whence $\frac{x^2}{y^2}=\frac{a^3}{b^3}$, which is the property which characterises FAGNANI'S point.

I will terminate this paper, by giving some remarkable properties of confocal ellipses and hyperbolas.

Lemma.—Let XY be the directrix of an ellipse, and P any point, we have by a property of the ellipse

$$HP : PX :: e : 1$$

e being the excentricity,

$$\therefore PX=\frac{HP}{e}$$

Therefore, calling CN, x, we have, when the point is at the extremity of the minor axis,

$$e : 1 :: \text{HB} : \text{BO}$$
$$:: \text{HB} : \text{CN}+\text{PX}$$
$$:: a : x + \frac{\text{HP}}{e}$$

Therefore $a = ex + \text{HP}$

and $\text{HP} = a - ex.$

This equation to the ellipse may often be useful.

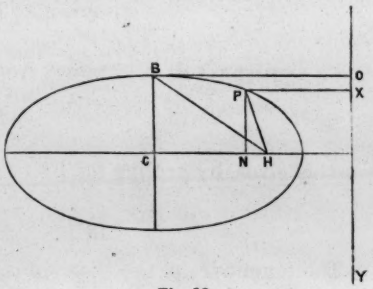


Fig. 13.

Theorem A.—If an ellipse and hyperbola are confocal, the line from the focus to the point of intersection equals the distance between the Vertices. Let S, H, be the foci, P the point of intersection, a, b, the semi-axes of the ellipse; A, B, those of the hyperbola, and V its Vertex,

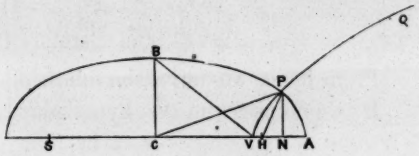


Fig. 14.

$\text{SP}+\text{HP}=2\text{CA}$, and $\text{SP}-\text{HP}=2\text{CV}$

$\therefore\ 2\text{HP}=2(\text{CA}-\text{CV})=2\text{VA}$

$\therefore\ \text{HP}=\text{VA}=a-\text{A}.$

The distance CH, between the centre and focus is usually expressed by ae, or e times the semi-axis major. But since in theorems concerning two or more confocal conics, CH is the only invariable line, it is convenient to denote it by unity. We will therefore in the sequel suppose CH, or $ae=1$; and, therefore $e=\frac{1}{a}$. It must be borne in mind that the condition of confocality gives the following relation between the axes,—

$$a^2-b^2=1=\text{A}^2+\text{B}^2.$$

Theorem B.—The same suppositions being made as in the last theorem, the co-ordinates of the point of intersection have the values

$$x=\text{A}a \quad y=\text{B}b.$$

And CP the central distance of the point of intersection, is equal to VB, which also has the value,—

$$\sqrt{\text{A}^2+a^2-1}.$$

For we proved in the last theorem that

$$\text{HP}=a-\text{A}, \text{ but by the Lemma } \left(\text{putting } e=\frac{1}{a}\right),$$

$$\text{HP}=a-\frac{x}{a}$$

Therefore, $\frac{x}{a}=\text{A}$, or $x=\text{A}a$.

It remains to find the value of y.

Take the equation to the ellipse $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$, and substitute for x its value Aa,

$$\therefore\ A^2+\frac{y^2}{b^2}=1,\ \therefore\ \frac{y^2}{b^2}=1-A^2=B^2,\ \therefore y=Bb.$$

And a similar result is obtained from the equation to the hyperbola, viz.—

$$\frac{x^2}{A^2}-\frac{y^2}{B^2}=1.$$

For this gives, by putting for x its value Aa,

$$a^2-\frac{y^2}{B^2}=1,\ \therefore\ \frac{y^2}{B^2}=a^2-1=b^2\ \therefore y=Bb.$$

The remainder of the theorem is thus demonstrated,—

Since $x=Aa$, and $y=Bb$,

$CP^2=x^2+y^2=A^2a^2+B^2b^2$

$=A^2a^2+(1-A^2)(a^2-1)=A^2+a^2-1=A^2+b^2$

$=VB^2$; and therefore $CP=VB$.

From hence we derive the following remarkable property of confocal conics:—

If two ellipses and two hyperbolas have all of them the same foci, and intersect in four points, k, l, m, n, forming a curvilinear quadrilateral, the straight lines lm, kn, which form the diagonals of this quadrilateral, are equal to each other.

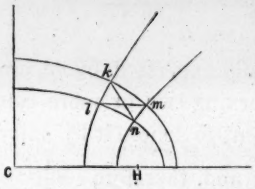


Fig. 15.

Demonstration.—Let the semi-axes of the ellipse and hyperbola *nearest the centre* be called a, b, and A, B; and let those of the ellipse and hyperbola *farthest from the centre* be called a', b' and A′, B′.

Let the co-ordinates of the point l be x and y.

those of $m \ldots x_1 \ldots y_1$

those of $k \ldots x_2 \ldots y_2$

those of $n \ldots x_3 \ldots y_3$.

Then the square of the diagonal $lm=(x_1-x)^2+(y_1-y)^2$

And of the diagonal $kn=(x_3-x_2)^2+(y_3-y_2)^2$.

What we have to prove, therefore, is that these two expressions are equal.

Now, since the point l belongs to the ellipse whose semi-axes are a, b, and also to the hyperbola whose semi-axes are A, B, we have

$x=Aa \quad y=Bb$. And for similar reasons,

$x_1=A'a' \quad y_1=B'b'$

$x_2=Aa' \quad y_2=Bb'$

$x_3=A' \quad ay_3=B'b$

Therefore, $xx_1=Aa\times A'a'$

And $x_2x_3=Aa'\times A'a$;

And therefore, $xx_1=x_2x_3$, each side being the product of the four semi-axes major.

In a similar way it is shown that $yy_1 = y_2y_3$, each side being the product of the four semi-axes minor.

Now, we wish to prove the equality of the expressions

$$(x_1-x)^2+(y_1-y)^2 \text{ and } (x_3-x_2)^2+(y_3-y_2)^2,$$

Subtract from them respectively the quantities

$$-2(xx_1+yy_1) \text{ and } -2(x_3x_2+y_3y_2)$$

which we have just proved to be equal.

The remainders will be $x^2+x_1^2+y_1^2+y^2$,

And $x_2^2+x_3^2+y_2^2+y_3^2$.

And now it is required to prove that these two remainders are equal.

But we have proved in theorem B, that

$$x^2+y^2 = A^2+a^2-1\text{; and by similar reasoning,}$$
$$x_1^2+y_1^2 = A'^2+a'^2-1.$$

Therefore $(x^2+y^2)+(x_1^2+y_1^2) = -2+$the sum of the squares of the 4 semi-axes major.

And by similar reasoning, $(x_2^2+y_2^2)+(x_3^2+y_3^2)$ is equal to the same quantity. Therefore the two remainders are equal; and therefore the theorem is demonstrated.

From this theorem several others may be deduced, by giving extreme values to the four curves.

In the first place, if the two ellipses are drawn infinitely near to each other, and likewise the two hyperbolas infinitely near to each other, then, because confocal conics always intersect at right angles, the small quadrilateral formed by the four intersections will be a rectangle, and of course its diagonals will be equal.

Next suppose that the axis minor of the first ellipse is infinitely diminished, the quadrant of the curve will be reduced to the straight line CH, extending from the centre to the focus. At the same time let the semi-axis *major* of the first hyperbola be infinitely diminished, and the vertex of it will then coincide with the centre, and the curve itself will become a straight line in the direction of the axis minor produced to infinity. The four curves will thus be reduced to one ellipse and one hyperbola, and two rectangular straight lines. The quadrilateral figure then becomes BCVP (see figure 14), and our theorem asserts that in that case CP = BV, the truth of which was independently proved in theorem B.

Now, let the other hyperbola also have its axis *minor* infinitely diminished, its vertex will then coincide with the focus, and the curve will be reduced to the straight line HA produced to infinity. The line CP then becomes CA, and BV

becomes BH (see fig. 14). Our theorem asserts, that in this case CA=BH, the truth of which is otherwise manifest.

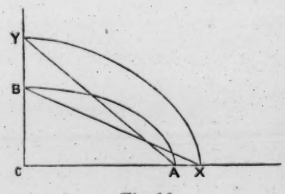


Fig. 16.

Again, let CA, CB, and CX, CY, be the semi-axes of the two confocal ellipses, but let the confocal hyperboles be reduced as before to the straight lines CBY (produced to infinity), and AX (produced to infinity), the intersections of the first hyperbola with the ellipses will be B and Y, those of the second A and X. The diagonals will become the lines BX and AY; and our theorem asserts that in this case BX = AY, which may be proved independently as follows:—

Let the semi-axes of the smaller ellipse be a, b, and of the larger one α, β.

Since they are confocal, $a^2-b^2=\alpha^2-\beta^2$, and therefore

$$a^2+\beta^2=\alpha^2+b^2.$$

But $a^2+\beta^2=\mathrm{AY}^2$, and $\alpha^2+b^2=\mathrm{BX}^2$.

Therefore $\mathrm{AY}=\mathrm{BX}$.

This theorem may be thus enunciated:—

"If the alternate vertices of two confocal ellipses are joined, the lines joining them are equal."

The co-ordinates of (x, y) the point of intersection of a confocal ellipse and hyperbola, may also, if preferred, be readily deduced from first principles, as follows:—

Given the confocal ellipse and hyperbola whose equations are $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$ and $\frac{x^2}{\mathrm{A}^2}-\frac{y^2}{\mathrm{B}^2}=1$, with the condition that $a^2-b^2=\mathrm{A}^2+\mathrm{B}^2=1$ to find the values of x and y the co-ordinates at the point of intersection?

The equations give respectively $y^2=b^2-\frac{b^2}{a^2}x^2$ and $y^2=\frac{\mathrm{B}^2}{\mathrm{A}^2}\cdot x^2-\mathrm{B}^2$. Equating these values of y^2, we find $\mathrm{B}^2+b^2=x^2\left(\frac{\mathrm{B}^2a^2+b^2\mathrm{A}^2}{\mathrm{A}^2a^2}\right)$. Now, substituting $1-\mathrm{A}^2$ for B^2, and a^2-1 for b^2, we find that $\mathrm{B}^2+b^2=a^2-\mathrm{A}^2$, which is also equal to $\mathrm{B}^2a^2+b^2\mathrm{A}^2$, and therefore may be omitted on both sides of the equation, which reduces itself to $1=\frac{x^2}{\mathrm{A}^2a^2}$ whence $x=\mathrm{A}a$.

Additional Note.—In the second page of this Memoir it is said, that the total deviation tends to increase without limit. To this it may be objected, that the successive deviations may possibly form a diminishing series, having a finite sum. But it can be easily shown that two successive deviations, when they are small, are (very nearly) equal to each other. Moreover, after diminishing to a certain extent, the deviations increase again, having one maximum and one minimum value in the course of one entire revolution round the circle.

XXV.—*On the Influence of Weather upon Disease and Mortality.* By R. E. Scoresby-Jackson, M.D., F.R.S.E., F.R.C.P., Lecturer on Materia Medica and Therapeutics at Surgeons' Hall, Edinburgh. (Plates XIV.–XVIII.)

(Read 2d February 1863.)

The subject to which I have to invite the attention of the Society this evening is one of no modern origin, the name of Hippocrates, amongst others of the fathers of medicine, being commonly associated with it. There is, indeed, perhaps no branch of medical inquiry whose history dips more deeply into the obscure pages of antiquity. The influence of weather upon disease and mortality has been acknowledged as a potent external force in every age, from that eminently speculative and credulous period when physicians professed to receive their diagnostic as well as their therapeutic inspirations from the stars, down to our own day. And yet there is perhaps no question in the whole cycle of medical sciences which has made slower progress than the one we have now to consider. People believe that the weather affects them. They speak of its influence, sometimes commendingly, more frequently with censure, on the most trivial occasions; but beyond a few commonplace ideas, the result of careless observation, or perhaps acquired only traditionally, they seldom seek a closer acquaintance with the subject. Our language teems with medico-meteorological apophthegms, but they are notoriously vague. The words which are most commonly employed to signify the state of the weather at any given time, possess a value relative only to the sensations of the individual uttering them. The general and convertible terms—bitter, raw, cold, severe, bleak, inclement, or fine and bracing, convey no definite idea of the condition of the weather; nay, it is quite possible that we may hear these several expressions used by different persons with reference to the weather of one and the same place and point of time. In order, then, to render medico-meteorological researches more trustworthy, we must be careful to employ, in the expression of facts, such symbols only as have a corresponding value in every nation.

As a matter of purely medical inquiry, the influence of weather is also too frequently neglected. So true is this, that when we examine the literature—at least the modern literature—of the subject, we find it to be most meagre, and very few are the statistics which we meet with to guide us in a further research. When I say, that in a medical point of view the influence of weather has been disregarded, I speak relatively to the amount of labour bestowed upon other branches of medical science; and I do not for a moment ignore the valuable contributions to this department which have been made from time to time by physicians of distinction. In France, Belgium, Germany, Italy, and America, as well

as in the United Kingdom, there have been many physicians during the present century, who by their labours have enriched this peculiar branch of medicine; and if I do not frequently quote their writings, it is neither from a want of respect to them individually, nor from a desire to underrate what they have accomplished, but simply because my time is very limited; and, further, because I think it will tend far more to the progress of the inquiry to pursue my own researches independently of any other investigations. It is well known that the influence of external causes upon the constitutions of living creatures differs materially with locality. These causes are not only numerous of themselves, but they are moreover capable of producing an infinite variety of results, according to their several combinations. Their effects are as distinct in different countries, as are the features of men of different nations. Therefore, conclusions derived from investigations pursued here can have no dependence upon the knowledge acquired by physicians in other countries; nor can any discrepancy which may be observed between the results of such several investigations serve to impugn the accuracy of individual deductions. It is quite possible that the results evoked by CASPER in Berlin, QUETELET in Brussels, BOUDIN in Paris, EMERSON in Philadelphia, and GUY in London, as well as those by the Registrars-General of the United Kingdom, may differ widely in many essential points, and yet the inferences of each observer be correct in themselves. The same may be said of the researches of Sir JAMES CLARK, CLESS, EDMONDS, EMERSON, FOISSAC, FRANCIS, FUCHS, HAVILAND, HALLER, HIRSCH, KEITH JOHNSTON, LOMBARD, MÄDLER, MARC D'ESPINE, MARTIN, MEYER, MILNE-EDWARDS, MÜHRY, RANSOME, RIGDEN, SCHÜBLER, TRIPE, VILLERMÉ, and of many other careful observers. My object is to examine the relations which exist between the weather and the health of a community as closely as I may find it practicable to do so; and in pursuing this inquiry, my desire is to divest myself of all foregone theory, and to make the facts which I have collated speak for themselves.

We have the strongest indication of the utility of such investigations in the fact, that, whether we do or do not possess a knowledge of the etiological and therapeutic influence of meteorological phenomena, we invariably act as if we were most intimate with the subject. Delicate persons crave for, and physicians often recommend, change of climate, which in many instances is a term convertible with change of weather, though it often means nothing more than change of scenery and of mental or physical occupation. If it be a good thing for a sick man to change his residence, it must be a proper thing for him to know what it is that he is avoiding, and what it is that he is to acquire in exchange for it in another place This must remain an exceedingly difficult question to solve, until we have statistics from every health resort, showing the correlations of weather and disease in each. We are not to assume that because certain conditions of weather, as indicated by meteorological instruments, in this country are opposed to recovery from certain

diseases, that, *therefore*, patients so suffering are not to be sent into any country where meteorological instruments afford exactly or even nearly parallel readings. In other words, in estimating the value of a foreign climate, or the different climates of our own country, we are not to depend so much upon a comparison of the meteorological data of the several places as upon the relations subsisting between the meteorological data and the prevalent diseases and death-rate of one and the same locality. Each spot of ground aspiring to the reputation of a health resort must first have this problem solved for it, and then we may with greater safety institute a general comparison. All that we can do in our present investigation is to find out, if it be contained in our data, what is good and what is objectionable in the climate of certain localities in Scotland, as evidenced by the general death-rate, and by the mortality from several causes. This will not in any way affect the character of the climates of Torquay, Bournemouth, Algiers, or Rome, except in so far as there may have been a line of investigation pursued in any of these places parallel with that now under consideration, so that a comparison may be instituted between the two; for, to argue, that because a given condition of temperature, atmospheric pressure, and humidity in Scotland is accompanied by a certain ratio of mortality, therefore, meteorological data being equal, the same death-rate will be observable in Torquay or Madeira, would be most fallacious: *all other things* being equal, the death-rate would also coincide; but it requires much more than mere meteorological analogy to establish such a parallelism.

But the present inquiry may serve another, and perhaps still more important, purpose. By far the greater number of cases requiring medical treatment do not involve the consideration of change of residence; nevertheless, I venture to assert that in all of them the weather plays an important part; and it cannot be otherwise than right for the physician to know whether he has, in the atmosphere around his patient, a foe or an ally in the treatment of his malady. In medical prognosis, a knowledge of the influence of weather is of essential service; and as an agent operating upon the therapeutic action of drugs, weather forms a most important study. I do not pretend that this is by any means an exhaustive inquiry; nor have I strained my facts to meet any so-called natural laws. On the contrary, the facts speak for themselves, sometimes making positive, sometimes negative assertions, and often enough hovering between the two, leaving us as much in doubt as before, but with a stimulus to deeper research into the influence of those numerous external agencies which are under the immediate control of the Great First Cause of all.

An inquiry into the causal relations subsisting between weather and disease is beset with a multitude of difficulties. In the first place, we ought not to attribute to the weather any effect upon the mortality of a given population, until we have abstracted all other causes which might have operated in a similar manner,

and to which such effect might altogether or in part be due: a task which is not very readily accomplished. Again, we cannot, if we would attain a rigid accuracy, attribute fluctuations in the death-rate to vicissitudes of weather, until we obtain a uniform climate over the entire area of observation, and this we shall never acquire. It may perhaps be objected to the results of my investigations, that it is not fair to apply the average of the climate of all Scotland to the death-rate of the eight larger towns; that the towns have a climate distinct from that of the rural and insular districts, and that each town has one of its own. That is quite true; but it is an objection which may with equal propriety be urged against the application of the climate of any large town to the mortality of its several parishes, the particular climate of each of which may, and often does, differ from that of its neighbour. But I prefer to consider the town districts alone, because it is in them that we meet with the mass of disease and the multiplied mortality; and as to the applicability of the general climate to a local death-rate, we may regard it in this way, that the climate of the towns is a climate within a climate, and that, whatever difference there may be between the containing and the contained, any modification of the larger must in a corresponding manner affect, if not in degree, at least in kind, the smaller.

Another objection might suggest itself in the returns of the cause of death made to the Registrar-General. It may be said that many of these returns are, at least, inexact; as, for example, when it is stated that death was caused by dropsy, the accumulation of fluid being merely a symptom of the real disorder: or, where the certificate tells only half the truth, as when a person who is dying of one disease is accidentally cut off by an intercurrent attack of another kind which would not have proved fatal but for the moribund condition of the patient at the time when it took possession of him; in such a case one class of disease is robbed of a victim which another gains by an adventitious circumstance. These and many other objections might be raised against investigations into the influence of the weather if we would be satisfied with nothing short of logical exactness; but we may undoubtedly arrive at an approximate knowledge of its effects if we are careful to avoid error in the main features of the inquiry.

The influence of external causes upon the constitutions of living creatures varies with locality, the variety depending upon the character of the causes, whether individually or combined. In a general way, these causes may be classed into two leading groups, as in the following order:—

A. Universal Causes, affecting Nations, as,—

1. *Position of a Country relative to*—The Equator—water in motion (*e.g.*, the sea with its currents; rivers, springs, extensive lakes)—stagnant waters (*e.g.*, canals, shallow lakes, marshes)—mountains, forests, arid plains, fields of ice.
2. *Aspect of a Country.*—General elevation and configuration, geological structure, physical and chemical properties of its soil, state of cultivation.
3. *Atmospheric Phenomena.*—Temperature, barometric pressure, direction and force of winds, humidity, insolation.

B. Local Causes, affecting Districts and Individuals, as,—

1. *Meteorological Phenomena*—Abnormal heat or cold, abnormal drought or humidity, abnormal fluctuations of the barometer (*i.e.* of atmospheric pressure), pernicious winds, ozone, electricity, diurnal phenomena, seasonal changes.
2. *Habitation*—Situation (town or country, on the coast or inland), elevation, construction, drainage, ventilation, heating, lighting, overcrowding.
3. *Dietetics*—Price of food, quality of food, quality and quantity of potable water, and of water for all domestic purposes.
4. *Personal*—Dress, occupation, habits and pursuits.

Although it may not be advisable to consider these several modifying causes in detail, nevertheless it is necessary that a passing glance should be accorded to them, in so far as they relate to this country.

Scotland, in its mainland, extends between 54° 38′, and 58° 40′ 30″ of north latitude, and 1° 46′, and 6° 8′ 30″ of west longitude. Including the circumjacent islands its limits are wider. The length of the mainland between its extreme points is 276 miles. Its breadth varies so greatly that no general idea can be given of it in one sum; it ranges from about 30 to 175 miles. In its general outline and configuration, Scotland is very remarkable. On glancing at a map of the country, the attention is at once arrested by the peculiar indentations of the coast line. In several situations the land is almost bisected by the prolongation of the sea into its interior, forming what are called Firths and Lochs, which serve to increase very considerably the shore of the country. The coast line, followed in all its sinuosities, occupies probably more than 3000 miles; but taking only the larger inlets of the sea into consideration, the circumference is probably about 2500 miles, which gives one mile of seaboard to every eleven square miles of surface; the estimated area of Scotland, inclusive of the Islands, being 31,324 square miles, or 20,047,360 acres. The ratio of seaboard to area over the whole of Europe is about one to twenty-five; Greece and Denmark being the only countries which approach Scotland in respect of proportional extent of coast. The islands of Scotland constitute about one-ninth of the entire area. It is obvious from these facts, that the sea, as an external cause operating upon the constitutions of the inhabitants, must be in the highest degree potent. And it is fortunate for Scotland that the influence of the sea is benign—unlike its action on the ice-bound shores of Labrador. It owes its mitigating influence, so far as Scotland is concerned, to the prevalence of westerly winds across its waters, tempered by the Gulf-stream; whereas, on the opposite shores of the Atlantic, the cold counter (Arctic and Hudson Bay) currents have a directly opposite tendency.

Besides the firths and marine lochs adverted to, Scotland possesses also many inland lakes, which, although not of magnitude comparable with those of the New World, tend materially to increase the aqueous element of its physical geography. The larger river-basins add also to the bulk of water in and around the country; in short, there is perhaps no point in Scotland more than a few miles

from a large body of water, and probably none more than forty miles distant from the sea. The same may be said with respect to the proximity of mountains; for there is scarcely any point commanding an extensive general view, from which a range of mountains may not be seen. There are five principal mountain chains, all of which assume a direction from N.E. to S.W. Besides these there are many detached groups, all of which exert a peculiar influence upon the climate. Forests do not form a special characteristic of Scotland, nor is there any barren plain within or near the country to modify the condition of the atmosphere in its passage across the land. But the ice-fields of arctic regions, lying not very far northward of Scotland, do probably exert a modifying power.

The geology of Scotland is one of the most striking features of the country; and that the structure of the land, together with the physical and chemical characteristics of the soil upon its surface, exercises a powerful influence upon the distribution of disease, I do not for a moment doubt; so much, indeed, am I impressed with the belief of this, that I have been for some time collecting materials, with a view of showing, more distinctly than has hitherto been done, the relations which these circumstances bear to each other.

The following table will serve to indicate the condition of the inhabitants of Scotland, during the several years under investigation, with respect to other external causes which might possibly divide with the weather the responsibility of determining the death-rate from all causes, or from any particular disease.

TABLE SHOWING THE AMOUNT AND DETAILS OF THE POOR-LAW EXPENDITURE IN EACH OF THE YEARS FROM 1852 TO 1861 INCLUSIVE; ALSO THE STATE OF THE FIARS-PRICES OF THE COUNTY OF EDINBURGH FOR THE CROPS FROM 1855 TO 1861 INCLUSIVE:—

Year.	1852.	1853.	1854.	1855.	1856.	1857.	1858.	1859.	1860.	1861.	Average.
Poor-Law.	£	£	£	£	£	£	£	£	£	£	£
Relief of Poor on the Roll,	401,954	411,135	428,708	461,243	486,689	492,118	496,297	512,751	518,546	531,233	474,967
Relief of Casual Poor, .	25,986	24,114	24,386	27,356	22,188	20,869	27,915	25,752	22,218	24,118	24,490
Medical Relief, . . .	21,436	21,737	27,874	27,166	24,008	24,205	24,948	25,691	26,738	26,908	25,071
Management, ...	51,644	52,352	56,068	58,767	61,462	63,142	66,307	67,166	67,048	67,717	61,167
Law Expenses, . . .	13,266	13,036	9,780	10,290	8,474	7,637	7,165	9,753	8,750	7,975	9,613
Poor-House Buildings, .	21,186	21,644	25,850	20,605	24,847	27,277	18,066	16,250	19,973	25,948	22,165
General Sanitary Measures,	393	532	6,259	6,355	1,677	1,122	...	...	...	...	1,634
Total Expenditure, .	535,865	544,550	578,925	611,782	629,345	636,370	640,698	657,363	663,273	683,899	618,210
Fiars-Prices.	s. d.	s. d.	s. d.	s. d.	s. d.	s. d.	s. d.	s. d.	s. d.	s. d.	s. d.
Wheat, 1st, ...	...	...	...	70 9	40 0	38 4	40 1	44 8	47 8	56 4	48 3
,, 2d, ...	...	...	...	68 0	35 0	35 6	37 0	42 0	44 8	54 0	45 2
Barley, 1st, ...	...	...	...	40 6	36 4	27 3	31 7½	37 6	40 5	31 3	34 11¼
,, 2d, ...	...	...	...	38 0	32 0	25 0	29 0	35 0	38 0	29 0	32 3
,, 3d, ...	...	...	...	36 0	28 0	22 6	27 0	32 6	35 0	27 6	29 9
Oats, 1st, ...	...	...	...	29 6	23 0	22 6	23 0	25 10	29 3	25 4	25 5
,, 2d, ...	...	...	...	27 6	20 0	20 6	21 0	22 6	27 0	23 6	23 1
Pease and Beans, . .	...	...	...	45 0	37 6	37 3	39 10	43 8	40 7	42 6	40 10
Oatmeal, ...	...	...	...	21 3	19 2	16 5½	16 8¾	18 9	21 5½	18 9	18 11¼

There remains, then, only one more subject for preliminary consideration, and that is, the sources of the different data employed in the following pages. The meteorological data are taken from the collected returns from the stations of the Meteorological Society of Scotland, as reduced by the Astronomer-royal. The stations of the Society have a mean latitude of 50° 30′ N.; a mean longitude of 3° 4′ W.; and a mean elevation of 222 feet nearly. It will be observed that the meteorological data are deficient in two points, namely, concerning electricity and ozone. Unfortunately, I have no means of applying these subtile agencies to the mortality of the years under examination; with respect to electricity, indeed, I have no information whatever; and concerning ozone, I have nothing trust-worthy. It is true that the Meteorological Society's reports contain the results of observations made with the usual test-papers in different parts of the country; but I submit, with all deference, that until the chemistry of ozone is more fully understood, its physiological action cannot be accurately defined. So long as it is left to each observer to determine the amount of ozone present at his station by the varying depth of colour on a slip of paper, our knowledge of the true quantity present must depend upon very slender evidence, and consequently be of very questionable accuracy. It is quite possible that six different observers might, with exactly the same indication on the test-paper, refer the amount of ozone present to as many different shades on the reference paper. Whether the paper itself affords a true indication of the presence of ozone, and to what extent, in the atmosphere, is a disputable matter. At all events, under existing circumstances, I should hesitate in comparing the ozone returns with the death-rate.

With respect to the humidity or dryness of the atmosphere, I have employed only three columns, showing, respectively, the number of rainy days, the amount of rain in inches, and the degree of saturation, as deduced by Mr GLAISHER: full saturation = 100. I have therefore omitted the readings of the dry and wet bulb thermometers, the temperature of the dew-point, and the elastic force of vapour. I have also omitted from the tables, although plotted in the diagrams, the absolute highest and absolute lowest temperatures at any of the stations; these are exceptional records, and can have no general application to the subject of the present inquiry.

The mortality tables are constructed from the returns made by the Registrar-General. The period over which my investigations extend is six years, namely, from 1857 to 1862 inclusive. This, I conceive, is quite long enough to indicate the relationship existing between the weather and mortality in non-epidemic years. I would, however, have made the period seven years, by including 1856, but I found that the meteorological data for that year were not trust-worthy; a circumstance arising from the newness of the Society, the inexperience of the observers at many of the stations, and a want of proper correction for the errors of the instruments then employed.

The absolute facts concerning the meteorology and mortality of the seventy-

two months are derived as already described ; but for the arrangement and calculations in the several tables, and for the inferences therefrom, mentioned in the text, I am alone responsible. The inquiry is led into the influence of weather upon mortality from all causes, from all specified causes, from zymotic diseases, from typhus, from scarlatina, from diarrhœa, from tubercular diseases, from phthisis pulmonalis, from diseases of the respiratory organs, from bronchitis, and from pneumonia, at all ages; and from all causes at four different periods of life—namely, under five years of age, between five and twenty, between twenty and sixty, and from sixty upwards.

In order to simplify the comparison of the meteorological with the mortality tables, and to render the fluctuations of the death-rate more distinct, I have reduced the number of deaths in every case to the ratio *per* 100,000, living in the eight larger towns at the time when the deaths were recorded, taking the estimated population for each of the six years as the standard of reference. Whether I have obtained a strictly correct estimate of the population of the eight towns or not, I cannot positively say; that given in the reports of the Registrar-General required considerable correction after the taking of the census in 1861. In consequence of this alteration, I had to recalculate my tables. In their present form the tables are calculated upon the following basis:—

Year.	Population of the Eight Large Towns.	Deaths from all Causes in the Eight Large Towns.
1857, ...	843,902	23,361
1858, ...	853,830	23,420
1859, ...	863,761	22,345
1860, ...	873,686	26,028
1861, ...	883,748	23,130
1862, ...	893,850	24,965
Average,	868,796	Total, 143,249

In the mortality tables, each of the months of thirty-one days is reduced to the value of thirty days, and the death-rate of February of each year is raised to the same value, so as to have uniformity over the whole seventy-two months.

The red dots upon the map indicate the situations of the meteorological stations; the red lines, the positions of the eight large towns, namely, Glasgow, Edinburgh, Aberdeen, Dundee, Perth, Greenock, Paisley, and Leith.

The diagrams which I have constructed to illustrate the paper, are, I venture to believe, not without considerable value. They present to the eye at one glance, the whole scheme of the investigation, and will probably leave a more vivid impression upon the minds of those who care to examine them than would result from an unaided examination of the tables, or a perusal of the text.

Of the three larger tables, the first (A) is arranged to represent a gradually descending ratio of mortality, the several months of the six years being placed in the order of the death-rate,—that in which the greatest number of deaths occurred

being at the top, that in which the lowest mortality took place being at the foot of the table. Opposite the mortality column are placed the several meteorological readings and deductions of the corresponding months. The columns are then divided into four distinct sections, each comprising eighteen months, the means of which are offered for comparison with each other, and with the means of the seventy-two months which are given alone in every third column. To have carried this table out to the extent of showing, in a similar manner, the order of the death-rate from the several classes of disease and individual diseases, as in the other tables, would have demanded more space than could reasonably be accorded. The materials for such extension, however, are given, and the arrangement might easily be made.

The second table (B) is constructed to represent the meteorology, and the death-rate from all and several causes at all ages, and from all causes at several ages, in the consecutive order of the months in each year: the means and totals of each year are given in separate columns, and the last column shows the means of the six years. The third table (C) is arranged for the purpose of comparing the meteorology and mortality of the several corresponding months of the different years, the mean of each of the 420 shorter columns being calculated in order to show more distinctly the character of the various deviations. I may also mention that each table was calculated independently of the others; on this account the general averages occasionally differ to a very trifling extent.

I.—The Influence of Weather upon Mortality from all Causes.

In the following details, I shall endeavour to show, as succinctly as possible, the influence of weather upon mortality from all and several causes; but I cannot pretend to exhaust the information which the tables and diagrams contain. I shall content myself with pointing out the prominent features of the inquiry; the facts from which they are drawn being placed without reserve before the Society, the inferences which I deduce from them are open to criticism, and nothing can fulfil my own desires more fully than an exposure of error, whether of fact (*i.e.*, of calculation) or of reasoning.

Season.—The influence of season upon mortality is not very distinctly understood, especially with reference to certain individual causes of death, opinions differing widely as to the months which determine the maximum and minimum of mortality from such particular causes. Here I would draw attention to the difference between mortality and disease; we shall fall into error if we suppose that the season of highest mortality is always the season of greatest sickness. It not unfrequently happens that certain seasons which are characterised by a maximum of sickness are at the same time distinguished for their low rate of

mortality; and contrariwise, seasons which may be somewhat remarkable for the general health of the public, may, by their influence upon one or two classes present a high death-rate. It is a very difficult matter to obtain accurate statistics of the prevalence of disease over a large community. After collecting a mass of statistics of disease from several dispensaries and hospitals, with a view of comparing the rate of *morbility* with the rate of mortality as given in this paper, I was obliged, after much labour, to abandon the *morbility* statistics, as next to worthless. Therefore this paper points to disease only through mortality.

If we turn to Table A, and regard the position of the months in the several sections, we shall find that while certain of the months are widely distributed through the column, others are arranged more compactly; but in no instance are the six corresponding months of the different years encompassed by one section of the column. If we apply to the several sections (from above downwards) respectively, the names *maximum*, *major*, *minor*, and *minimum* of mortality, and arrange under each title the number of corresponding months found in the section, we shall at once see how many of the years approached to uniformity of mortality, and how many were exceptional.

Month.	Maximum Mortality.	Major Mortality.	Minor Mortality.	Minimum Mortality.
January,	4	1	1	...
February,	3	3	...	...
March,	4	1	1	...
April,	2	2	2	...
May,	...	3	2	1
June,	...	...	4	2
July,	...	1	2	3
August,	...	1	...	5
September,	...	...	1	5
October,	...	...	4	2
November,	1	5	...	...
December,	4	1	1	...
Total,	18	18	18	18

This table shows us the distribution of the months, but not their order as determined by an average of the six years; for this we must look to Table C, and taking from it the means of the several columns, showing the mortality from all causes during the six years, we shall find the order of the months to be as follows:—

	Month.	Death-rate.		Month.	Death-rate.
Maximum,	January,	265·3	Minor,	May,	219·5
	February,	257·4		June	208·6
	March,	249·8		July,	204·5
Major,	December,	247·9	Minimum,	October,	198·2
	April,	242·8		August,	189·4
	November,	237·1		September,	187·7

Mean 225·7

If we revert to the previous table, we find that January, March, and December, have a like distribution over the *maximum*, *major*, and *minor* sections, the determining years being in the *maximum*, and an exceptional year in each of the two following sections, and we might have expected that these months would all have preceded February, which only contributes three years to the maximum section; but in the latter table we find February in its true place, taking precedence of March and December. This arises from the exceptional years of the latter months having a much lower rate of mortality than any of the Februaries. Before proceeding to examine the meteorological data in detail, we have here an opportunity of testing the influence of weather in a general way. If the weather had anything to do with the placing of the exceptional years, we shall expect to find that in the months of January, March, and December, those years which contribute to the major and minor sections will be more temperate than those whose months enter into the maximum section. In the case of November, severity of weather ought to characterise the exceptional year, and there ought, moreover, to be some marked peculiarity in the meteorology of the exceptional August. It will be unnecessary to test the other months at this stage.

We begin with January, and instead of quoting figures, let us take the general remarks contained in the meteorological reports as our guide. The four Januaries of the maximum section are those of 1861, 1862, 1860, and 1857, in the descending order of their mortality; the exceptional January in the major section is that of 1859; that in the minor section, the January of 1858. The relations which the months of the different years bear to each other, will be still better understood in this form:—

Year.	Maximum. Mortality	Major. Mortality	Minor. Mortality	
1861,	304·1	...	...	
1862,	296·6	...	...	
1860,	280·8	...	...	Mean 265·3.
1857,	253·2	...	...	
1859,	...	243·4	...	
1858,	...	...	214·0	

January 1861.—(I quote from the Meteorological Reports) "From these returns we gather that the month of January was still, like so many of the preceding ones, below the average in temperature, though in a less degree; the barometrical pressure was unusually high; but otherwise there are no remarkable differences in the other meteorological elements." Dr STARK says of the same month,—"The intensely cold weather which set in about Christmas, and, after a partial intermission, recurred during the earlier part of January, exhibited the usual effect of low temperature in this country, in largely increasing the number of deaths."

January 1862.—Meteorological Report: "From these returns we gather that

in January 1862 the most remarkable features were the great depth of rain, the large number of rainy days, the large amount of cloud, and the small amount of sunshine,—each of these quantities being more extreme than ever before registered in a January month. The mean temperature was also high, though not to so great a degree." Registrar-General: "January was a comparatively mild month for the season, with a mean temperature rather above the average, an unusual amount of mild south-westerly breezes, a consequently greater fall of rain, and a greater degree of humidity of the atmosphere than is usual during that month. This mild weather was, however, often interrupted by the wind suddenly veering to the north and east, and blowing with a keenness all the more severely felt, and the more detrimental to the health of the people from the previous mildness."

January 1860.—Meteorological Report: "For the month of January we thus find that the mean temperature, though 1°·5 higher than that of the preceding months, is yet 1°·7 less than the average of former Januaries, constituting therefore a particularly severe month, as additionally manifested by the black-bulb temperature by day being 6°·2 less than the average, and by night 3°·3 less,—the hours of sunshine being less, and the amount of cloud rather greater. The mean humidity has also been greater, as well as the amount of rain, with an unusual preponderance of north-east wind." Registrar-General: "The past quarter, coming as it did after the wintry weather which prevailed during November and December, was one of the most severe which has been experienced in this country for at least thirty-four Januaries, if not for a much longer period."

Such are the reports of the weather of those months whose death-rate is above the average of the six Januaries; from this point the weather ought to improve.

January 1857.—Meteorological Report: "January. The weather was as nearly as possible an average in point of severity." Registrar-General: "During January the weather was generally open, though the month began and ended with a snow-storm." This month, although below the average mortality of the six Januaries, still contributes to the maximum section.

January 1859.—Registrar-General: "The weather during the quarter was mild for the season, but stormy and rainy to an extent scarcely remembered to have been equalled within the recollection of the oldest inhabitants. The winds, which, during almost all the quarter, were from the west and south-west, were unusually high, and brought much rain with them." The Meteorological Report is too extensive for quotation; in substance it is the same as the remarks of the Registrar-General. It should be mentioned, however, that whilst there was a remarkable increase of rain in the western counties, there was at the same time a remarkable deficit in the eastern counties.

January 1858—Meteorological Report: As showing the general mildness of the month, it may be mentioned that at Sandwick, wall-flower, stock, carnation, and borage were in flower, so as to yield a bouquet on the 1st of January, while the hepaticas were in flower on the 4th. At Aberdeen, the hazel and snowdrop were in flower on the 25th; and at Banchory House the *Rhododendron ponticum* was in flower on the 29th. The thrush was often heard singing during the month at Scourie, and the lark at Aberdeen." Registrar-General: "The weather during the first two months of the quarter was unusually mild and open; and though the mean temperature was gradually falling during January and February, it was not till the 1st of March that winter, with its frosts and snow, fairly set in over the country."

Thus far, then, whatever we may meet with in detail, we must be impressed with the fact that the general term of a "mild" or "open" January corresponds with a low rate of mortality, a "severe" January with a high rate of mortality.

We next proceed to consider the months of March, and, in order to avoid long quotations, if we find that the exceptional months were "mild," we may assume that the four months of the maximum section were more or less "severe."

Year.	Maximum Mortality.	Major Mortality.	Minor Mortality.	
1860, . . .	283·2	...	...	
1858, . . .	257·8	...	...	
1862, . . .	256·7	...	...	Mean 249·8.
1857, . . .	250·0	...	...	
1859, . . .	...	232·0	...	
1861, . . .	...	...	220·2	

March 1859.—Registrar-General: "This month has therefore been characterised, even more intensely than the last, by an unusual amount of west wind, a low barometer, and, on the western coast, abundant rain and equable temperature. On the eastern coast, the rains have been scanty, but the temperature high."

March 1861.—Registrar-General: "The months of February and March however, have both been above the average temperature; and as this increase of temperature has been attended with a greater fall of rain and a greater amount of humidity than usual, while there has been a diminution in the proportion of cold arid east winds, and a preponderance of high winds from the west and south-west, there has been a free circulation of air, and no such stagnation of the atmosphere as would allow the excessive moisture to become hurtful to the living inhabitants. Hence the mortality of February and March has been below the average of former years; and if the weather continues favourable, as it appears to be giving every indication of doing, the present year may prove, like the census year 1851, a year of low mortality, and of prosperity and heavy crops to the farmer." Here "mildness" characterises also the months of lower mortality.

We may test the months of December in a similar manner by ascertaining the character of the exceptional years.

Year.	Maximum Mortality.	Major Mortality.	Minor Mortality.	
1859, . . .	263·4	...	...	Mean 247·9.
1862, . . .	259·0	...	...	
1860, . . .	257·7	...	...	
1858, . . .	250·0	...	...	
1861, . . .	...	241·4	...	
1857, . . .	...	...	215·7	

December 1861.—Registrar-General: "From the returns it appears that the month of December, in the important feature of mean temperature, has been nearly normal, and thereby between three and four degrees warmer than the two last Decembers."

December 1857.—Registrar-General: "During the quarter the weather was unusually mild,—so much so, indeed, that even sprigs of hawthorn in full blow, and several spring flowers, were gathered at Christmas; and in the north, a second and abundant crop of cranberries was gathered the second week of December."

The months of November present only one exceptional year, and that alone contributes to the maximum section; it will be sufficient to ascertain whether it is remarkable for its "severity."

Year.	Maximum Mortality.	Major Mortality.	
1858, . . .	266·1	...	Mean 237·1.
1859, . . .	...	236·0	
1862, . . .	...	234·7	
1857, . . .	...	231·3	
1860, . . .	...	230·0	
1861, . . .	...	224·5	

November 1858.—Registrar-General: "During November, again, the mean temperature of the month was 2°.2 below the average, and severe, stormy weather, accompanied by keen frost and falls of snow, prevailed during the third week of the month."

We have, however, an anomaly in November 1861, in which, with the lowest death-rate of all the Novembers, we find the following description of the weather:—"Hence it appears that the month of November has been cold, wet, and windy, to an unprecedented degree. The barometer was lower, and more uniformly low than in any month of the last six years. The mean temperature was lower than in any November through the same time." It will be noticed, however, that there is not a great difference in the death-rate between the several years entering into the major section. And, lastly, concerning the months of August. One of these months only presents an exceptional death-rate; it is that of the year 1857. The months of September present also an exceptional month, likewise that of

1857, but not to so great an extent as is witnessed in the case of the aberrant August; and forasmuch as the cause in both instances is obviously identical, it will be sufficient to explain it in reference to August only.

Year.	Major Mortality.	Minor Mortality.	Minimum Mortality.	
1857, ...	224·1	...	...	
1858, ...	...	...	199·5	
1860, ...	...	...	192·4	Mean 189·4
1859, ...	...	...	178·7	
1861, ...	...	...	173·1	
1862, ...	...	...	168·4	

August 1857.—Registrar-General: "The mean temperature in August realised the very unusual height of 60°, and as July also, and the beginning of September, had mean temperatures higher than usual, bowel complaints (diarrhœa, dysentery, and cholera) became so prevalent and fatal, that instead of only 56 dying from these complaints in every hundred thousand persons, as in 1856, no fewer than 112 deaths occurred in a like population in 1857."

We proceed now to consider the influence of the several meteorological phenomena upon the mortality from all causes more in detail, and first we have to consider

The Influence of Temperature upon Deaths from all Causes.

Turning to Table A, we find that although the column of mean temperature does not increase in value so evenly as the column of mortality diminishes in value from above downwards, nevertheless over the whole of the two columns there is a distinctly inverse relationship; and if we compare the means of the four sections previously described, we find that the relationship existing between temperature and deaths from all causes is as follows:—

Mean of	Mortality.	Mean Temperature.
Maximum Section, ...	269·49	38·3
Major Section, ...	233·25	43·1
Minor Section, ...	214·26	49·5
Minimum Section, ...	185·85	54·3
Mean of the 72 Months,	225·71	46·3

To ascertain the difference of influence between a high and low mean temperature, I have constituted, out of the six, three factitious years; for the one, taking all the lowest temperatures of the six corresponding months; for another, the highest temperatures; and for the third, the mean of the six corresponding months. The death-rate of the three may be compared month by month. [Obviously, one disadvantage of such an arrangement is, that by associating the sequent months of different years we at once dissolve the continuity of effect which the weather exercises over mortality. It is important, in estimating the value of weather as an etio-

logical agent at any given time, that at least the character of the weather immediately preceding the period under examination should be ascertained, the influence of sustained heat or cold, for example, as will be shown hereafter, being remarkable. Nevertheless, when the period of comparison extends over an entire month, I think we may safely believe that the fluctuations in the death-rate—in so far as they are dependent upon weather at all—are dependent upon the weather of the particular month in which the deaths are recorded. This is not uniformly so Suppose, for example, that the fourth week of January were intensely cold, and the subsequent month of February comparatively mild, it is quite possible that a large number of deaths, caused by the cold of January, might fall to be registered in February; so that if either of the months were examined separately, an erroneous impression concerning the influence of temperature would result. If the periods of observation had been daily or weekly, I would on no account have separated them, because necessarily the ratio of deaths of one day, or even of one week, must often be, to a certain extent, modified by the weather of the previous days or week; but, I repeat, where each subject of comparison extends over a whole month, errors from such a cause as I have now explained must be very trifling. It is, however, to obviate such errors that I have constructed Table B, in which both the meteorological and necrological data are arranged appositely in the order of the sequence of the months of the several years. This explanation is also offered to objections which may be raised against the order of the months in Table A.] I need not mention the years from which the several months are taken; that will be seen on reference to the larger table:—

	January.		February.		March.		April.		May.		June.	
Months of Lowest Mean Temperature,	35°·5	280·8	34°·0	330·6	37°·8	256·7	41°·3	222·7	49°·1	225·4	52°·4	215·0
„ Highest, „	39·6	243·4	40·5	246·9	43·0	232·0	45·4	223·5	51·9	193·5	58·9	219·3
Mean of the Six Years, . . .	37·4	265·3	38·2	257·4	39·8	249·8	43·2	242·8	50·3	219·5	55·9	208·6

	July.		August.		September.		October.		November.		December.	
Months of Lowest Mean Temperature,	53°·8	192·3	54°·4	192·4	50°·2	182·5	44°·9	205·3	37°·1	234·7	34°·0	263·4
„ Highest, „	59·0	181·3	60·0	224·1	56·1	210·8	49·6	208·0	43·7	231·3	44·9	215·7
Mean of the Six Years, . . .	56·8	204·5	57·2	189·4	52·5	187·7	47·2	198·2	39·5	237·1	38·8	247·9

If this table speak truly, it leads to the conclusion, that for every diminution of mean temperature below 50° there is a corresponding increase of mortality; but that from mean temperatures above 50° a diminution is favourable to vitality, at least if the temperature have been for any length of time above 50°. In other words, mean temperature and mortality from all causes have an inverse relationship below 50°, a direct relationship above 50°. But it must be borne in

mind that the change is gradual: thus, in May, with a temperature of 51°·9 there is a mortality of 193·5, which is still below 225·4, the mortality corresponding with a mean temperature of only 49°·1. But if this be a rule, then the months of April and July are exceptional, the April with a lower mean temperature having also a slightly higher death-rate, the anomaly perhaps depending upon other meteorological elements. The April with the higher mean temperature and the lower death-rate had a mean barometric reading of 29·751, and a rain-fall of 3·20 inches; the other April had a mean barometric reading of 30·177, and a rain-fall of only 1·04 inches,—the one a low atmospheric pressure with a surplus humidity, the other a high rate of atmospheric pressure with a deficit of moisture. If I had selected the April of 1860, whose mean temperature is only 0·2° above the one in the table, namely 41·5°, the rule would have been sustained, the death-rate for that month being 290·2. The July with the lower temperature and lower death-rate had a mean barometric reading below the average of the six years with a rain-fall above it; the July with the higher mean temperature and death-rate had a barometric reading above the average of the six years with a rain-fall below the average. The July of 1857, with a mean temperature only one degree below that in the table, had a death-rate of 210·5.

Monthly Range of Temperature.—The relation of monthly range of temperature to the death-rate from all causes is also shown in Table A, the means of the four sections being as follows:—

Section.	Mortality.	Monthly Range of Temperature.
Maximum, ...	269·49	36·4
Major, ...	233·25	37·2
Minor, ...	214·26	39·6
Minimum, ...	185·85	33·9

If we adopt the same plan as with the mean temperature, constituting out of the six two excessive years, and comparing them with the mean, we have the following results:—

	January.		February.		March.		April.		May.		June.	
Months of greatest Monthly Range,	50°·1	253·2	56°·0	250·1	56°·0	257·8	61°·0	227·1	54°·7	210·4	66°·5	213·3
Months of least, „ „	21·7	296·6	29·0	246·9	27·0	283·2	32·2	290·2	31·2	228·4	27·3	215·0
Means of the Six Years, . . .	34·1	265·3	38·3	257·4	37·3	249·8	41·9	242·8	41·6	219·5	39·5	208·6

	July.		August.		September.		October.		November.		December.	
Months of greatest Monthly Range,	46°·5	210·5	53°·0	224·1	55°·0	194·6	47°·0	208·0	50°·0	231·3	40°·8	257·7
Months of least, „ „	26·2	192·3	25·1	173·1	27·7	165·0	29·6	210·4	26·5	230·0	22·7	259·0
Means of the Six Years, . . .	34·6	204·5	35·7	189·4	37·8	187·7	36·2	198·2	33·9	237·1	32·1	247·9

The two last tables, I think, point to this, that for three quarters of the year the relationship of monthly range of temperature and the death-rate from all causes is inverse—the greater the range the lower the mortality; but that during the months of July, August, and September, the relationship is direct—the greater the range the greater the mortality.

Mean Daily Range of Temperature.—The relationship as shown in Table A. is as follows:—

Section.	Mortality.	Mean of the Daily Ranges.
Maximum, ...	269·49	9·8
Major, ...	233·25	11·6
Minor, ...	214·26	13·0
Minimum, ...	185·85	13·8
Means, . . .	225·71	12·0

The result of constructing two excessive years out of the six is as follows:—

	January.		February.		March.		April.		May.		June.	
Months of greatest Mean Daily Range,	9°6	243·4	10°8	250·1	11°9	257·8	15°3	227·1	19°7	193·5	17°2	213·3
Months of least „ „	7·1	296·6	8·6	251·3	9·3	256·7	12·0	243·6	14·1	228·4	12·6	215·0
Means of the Six Years, . .	8·4	265·3	10·0	257·4	10·7	249·8	13·9	242·8	15·6	219·5	14·9	208·6

	July.		August.		September.		October.		November.		December.	
Months of greatest Mean Daily Range,	14°7	224·0	16°0	199·5	14°6	194·6	12°7	204·4	11°0	234·7	9°5	241·4
Months of least „ „	13·1	192·3	11·0	173·1	11·0	165·0	10·9	210·4	8·1	230·0	8·1	257·7
Means of the Six Years, . .	14·0	204·5	13·5	189·4	13·3	187·7	11·8	198·2	9·9	237·1	8·6	247·9

These tables indicate a similar relationship between mortality from all causes and the mean daily range of temperature as was noticed with respect to the monthly range of temperature, namely for three quarters of the years an inverse relationship, the greater the daily range the less the death-rate, but during the months of July, August, and September, a direct relationship, the greater the daily range of temperature the greater the mortality. It will be noticed in the latter table that March and November do not quite conform.

The Combined Influence of Temperature and Humidity.

It is difficult to obtain data for this inquiry, there being few months which, with equal mean temperatures, show at the same time a marked contrast in their hygrometric condition. I have, however, endeavoured in the following table to show the relative effect upon the death-rate of a dry and humid cold:—

Months.	Mean of the Six Years.				Dry Cold.				Humid Cold.			
	Mean Temp.	Rain-fall.	Humi-dity.	Mor-tality.	Mean. Temp.	Rain-fall	Humi-dity.	Mor-tality.	Mean Temp.	Rain-fall.	Humi-dity.	Mor-tality.
	°				°				°			
January, . . .	37·4	3·82	88	265·3	35·7	2·77	87	253·2	35·5	4·56	89	280·8
					39·3	2·98	86	214·0	39·6	4·21	87	243·4
February, . . .	38·2	2·32	87	257·4	39·3	1·54	89	250·1	39·5	3·32	88	228·1
					40·1	1·88	89	251·3	40·5	3·38	88	246·9
March, ...	39·8	3·55	86	249·8	38·4	3·52	86	283·2	37·8	3·63	88	256·7
					39·5	1·95	85	257·8	39·2	2·94	86	250·0
April, ...	43·2	2·11	82	242·8	41·5	1·18	82	290·2	41·3	3·20	80	222·7
					43·8	1·86	78	227·1	44·6	2·99	83	250·0
Means, . .	39·7	2·95	86	253·8	39·7	2·21	85·2	253·4	39·7	3·53	86·1	247·3

In this table there is a tendency to support the general belief that a *dry* cold is more fatal than a *humid* cold; the four Januaries, it will be seen, oppose the idea of such a law, the greater death-rate being with the two humid months; two of the Aprils also conflict with the generally accepted law, the humid month, even with a rather higher mean temperature, having the greater mortality. In the months of March, and again in one of the comparisons of the months of April, it will be noticed, that whilst the order of the months is correct as to the amount of moisture indicated by the rain-fall in inches, nevertheless, if the humidity as deduced by Mr Glaisher's tables had been strictly taken as the test, the order ought to have been reversed. But it often happens that, as in the instances above referred to, whilst the amount of rain which may have fallen during a month may be higher than the mean of the six corresponding months, nevertheless, the amount of moisture in the atmosphere, as exhibited by a reference to Mr Glaisher's tables, may be below the average humidity of the six corresponding months. It behoves us, then, to compare both of these indices to the amount of moisture in the atmosphere with the temperatures below the means of the six corresponding months separately, and this is done in the following tables:—

Months in which both the Mean Temperature and the Rainfall are below the Mean of the Six corresponding Months.

		Mortality.
January	1857, ...	253·2
„	1861, ...	304·1
February	1858, ...	237·7
March	1857, ...	250·0
„	1858, ...	257·8
„	1860, ...	283·2
April	1860, ...	290·2
	Mean, . .	268·0

Months in which the Mean Temperature is below, but the Rainfall above the Mean of the Six corresponding Months.

		Mortality.
January	1860, ...	280·8
February	1860, ...	330·6
March	1862, ...	256·7
April	1857, ...	243·6
April	1859, ...	222·7
...	...	...
...	...	...
	Mean, . .	266·9

Months in which both the Mean Temperature and Humidity are below the Mean of the Six corresponding Months.		Mortality.
January	1857,	253·2
February	1858,	237·7
„	1860,	330·6
March	1858,	257·8
April	1859,	222·7
	Mean,	260·4

Months in which the Mean Temperature is below but the Humidity above the Mean of the Six corresponding Months.		Mortality.
January	1860,	280·8
„	1861,	304·1
March	1862,	256·7
April	1857,	243·6
„	1860,	290·2
	Mean,	275·1

Here, then, we have conflicting results. Following the rain-fall as our guide to the hygrometric condition of the atmosphere, we conclude that a dry atmosphere promotes mortality; if we refer the humidity present in the atmosphere to the standard calculated by Mr Glaisher, we are forced to an opposite opinion. Let these results be estimated only at their true value; the data are too few to render them of very great importance, but they may serve for comparison with other investigations. It must be remembered, however, that it is the temperature that binds me down to so few data. I shall have occasion hereafter to inquire into the value of the relative quantity of moisture in the atmosphere separately.

Before passing from the consideration of the influence of temperature upon mortality from all causes, there is one other important feature which ought to be mentioned,—namely, the aggravating influence of continued cold. The growing mortality consequent upon a continued low temperature is seen in the following table, where it is contrasted with the means of the mean temperature and mortality of the six corresponding months:—

	November.	December.	January.	February.	March.	
Year 1860,	39°·1	34°·1	35°·5	34°·0	38°·4	Mean Temp.
	230°·0	257°·7	280°·8	330°·6	283°·2	Mortality.
Average of the Six corresponding Months,	237°·1	247°·9	265°·3	257°·4	249°·8	Mortality.
	39°·5	38°·8	37°·4	38°·2	39°·8	Mean Temp.

In like manner the evil effects of a continued high temperature may be seen in the following table, in which the mean temperature and mortality of the warmer months of 1857 are contrasted with those of the averages of the six corresponding months of the years 1857-62 inclusive:—

	June.	July.	August.	September.	October.	
Year 1857,	57°·4	58°·0	60°·0	56°·1	49°·6	Mean Temp
	213°·3	210°·5	224°·1	210°·8	208°·0	Mortality.
Average of the Six corresponding Months,	208°·6	204°·5	189°·4	187°·7	198°·2	Mortality.
	55°·9	56°·8	57°·3	52°·5	47°·2	Mean Temp.

The Influence of Vicissitudes of Atmospheric Pressure.

When we notice that the average monthly range of the barometer for the six years under examination is not more than 1·262 inches, it behoves us to remember how much is implied in the oscillation of the barometer even to the extent only

of a single line. It indicates either a vast increase or diminution of pressure upon every point of surface of the body,—a pressure which normally is equal to that of fifteen pounds of dead weight to the square inch, and yet, a pressure so wonderfully adapted to our necessities that we are unconscious of its presence. So that, although in dealing with this part of the inquiry we are confined, with very few exceptions, to movements within the thirtieth inch of the barometric scale, we are not to suppose that such limited movements are of trifling, but that they are of all-important value.

Mean Height of Barometer reduced to Sea-level and 32° F.

A comparison of the means of the four sections in Table C, between the mean height of the barometer and the mortality from all causes, leaves us in uncertainty as to the relations which these circumstances bear to each other. It is indicated in the following table:—

Section.	Mortality from all Causes.	Mean Height of Barometer.
Maximum Section, ...	269·49	29·800
Major Section, ...	233·25	29·868
Minor Section, ...	214·26	29·862
Minimum Section, ...	185·85	29·842
Mean, ...	225·71	29·843

The mean height of the barometer for the whole of each of the six years, as compared with the mortality for the corresponding periods, is shown as follows:—

Year.	Average Monthly Mortality from all Causes.	Mean Height of Barometer.
1860, ...	244·2	29·785
1862, ...	229·2	29·812
1857, ...	227·5	29·893
1858, ...	225·5	29·916
1861, ...	214·9	29·838
1859, ...	212·7	29·817
Means, ...	225·7	29·843

In both of these tables the lowest reading of the barometer corresponds with the highest death-rate. In both, the two lowest readings of the barometer correspond respectively with the highest and lowest death rates, with a minute fractional difference in the latter table, the second lowest reading being that of the mean of 1862. In the latter table the highest barometric reading corresponds with the mean of the mortality; in the former, the highest barometric reading stands opposite a much higher death-rate. The only point of resemblance between the two being that first mentioned, indicating a maximum of deaths with a minimum of atmospheric pressure. But we must not remain satisfied with the value of this indication alone.

In the next table we have three factitious years constituted,—the first, by those of the several corresponding months which possess the highest barometric readings; the second, by those which exhibit the lowest barometric readings; and the third, by the means of the six corresponding months:—

Months.	Highest Barometer.	Lowest Barometer.	Mean Barometer.	Mortality with Highest.	Mortality with Lowest.	Mean Mortality.
January, ...	30·065	29·529	29·813	214·0	280·8	265·3
February.	30·052	29·681	29·869	251·3	228·1	257·4
March, ...	29·852	29·507	29·701	257·8	220·2	249·8
April, ...	30·177	29·751	29·916	223·5	222·7	242·8
May, ...	30·070	29·810	29·923	225·4	228·4	219·5
June, ...	30·032	29·674	29·892	219·3	219·1	208·6
July, ...	30·050	29·735	29·851	181·3	192·3	204·5
August, ...	30·014	29·575	29·840	224·1	192·4	189·4
September, ...	29·979	29·722	29·845	195·9	177·7	187·7
October, ...	29·936	29·620	29·784	173·1	204·4	198·2
November, ...	30·115	29·855	29·881	231·3	236·0	237·1
December, ...	30·020	29·651	29·806	241·4	263·4	247·9
Means, ...	30·030	29·676	29·843	219·9	222·1	225·7

In this table there is evidently a good deal of conflict; but upon the whole year it corroborates the indications of the preceding tables, inasmuch as the average mortality with a low barometer is higher than that with a high barometer. But it is curious to observe that the average barometric reading of the whole term of seventy-two months corresponds with a higher necrological reading than either the absolute highest or the absolute lowest readings of the barometer, as is more distinctly seen thus:—

Mean of the Absolute Highest Barometric Readings,	30·030	Mortality,	219·9
,, Lowest ,,	29·676	,,	222·1
Mean of the whole Term of 72 Months, ...	29·843	,,	225·7

This, then, would indicate that high and low barometric readings are both more conducive to vitality than a medium reading; and, moreover, it serves to deepen the impression that a low barometer is more fatal than a high reading; because, although 29·843 is the exact mean of the whole term of seventy-two months, nevertheless it approximates nearer to the mean of the absolute lowest than to the mean of the absolute highest, for $30{\cdot}030 + 29{\cdot}676 \div 2 = 29{\cdot}853$.

In order to ascertain whether any month or season would give a stronger indication of the influence of atmospheric pressure upon the death-rate than is shown by the mere absolute highest and lowest barometric readings, and also whether season has any modifying power over such influence, I have classified as many of the corresponding months into separate groups as would in any way contrast, touching the circumstance of atmospheric pressure:—

	Year.	Barometer above Mean of Six corresponding Months.	Mortality.	Year.	Barometer below Mean of Six corresponding Months.	Mortality.
January,	1858	30·065	214·0	1857	29·698	253·2
	1859	29·864	243·4	1860	29·529	280·8
	1861	30·038	304·1	1862	29·686	296·6
	Average mortality,		253·8			276·9
February,	1858	29·998	237·7	1857	29·840	250·1
	1860	29·932	330·6	1859	29·709	246·9
	1862	30·052	251·3	1861	29·681	228·1
	Average mortality,		273·2			241·7
March,	1857	29·804	250·0	1860	29·639	283·2
	1858	29·852	257·8	1861	29·507	220·2
	1859	29·707	232·0	1862	29·698	256·7
	Average mortality,		246·6			253·3
April,	1858	29·942	227·1	1857	29·767	243·6
	1860	29·978	290·2	1859	29·751	222·7
	1861	30·177	223·5	1862	29·881	250·0
	Average mortality,		246·9			238·8
May,	1857	29·959	219·3	1858	29·823	210·4
	1859	30·046	193·5	1860	29·831	240·0
	1861	30·070	225·4	1862	29·810	228·4
	Average mortality,		212·7			226·3
June,	1357	30·020	213·3	1860	29·674	219·1
	1858	30·032	219·3	1862	29·733	215·0
	1859	29·934	188·6	...	...	...
	1861	29·961	196·4	...	...	...
	Average mortality,		204·4			217·0
July,	1858	29·887	224·0	1857	29·832	210·5
	1859	30·050	181·3	1861	29·619	204·4
	1860	29·988	214·8	1862	29·735	192·3
	Average mortality,		206·7			202·4
August,	1857	30·014	224·1	1860	29·575	192·4
	1858	29·943	199·5	1861	29·774	173·1
	1859	29·850	178·7	...	...	...
	1862	29·885	168·4	...	...	...
	Average mortality,		192·7			182·7
September,	1857	29·882	210·8	1859	29·722	177·7
	1858	29·898	194·6	1861	29·723	165·0
	1860	29·868	182·5	...	...	...
	1862	29·979	195·9	...	...	...
	Average mortality,		195·9			171·3

Year.		Barometer above Mean of Six corresponding Months.	Mortality.	Year.	Barometer below Mean of Six corresponding Months.	Mortality.
October,	1857	29·803	208·0	1859	29·667	188·0
	1858	29·892	205·3	1860	29·784	210·4
	1861	29·936	173·1	1862	29·620	204·4
	Average mortality,		195·5			200·9
November,	1857	30·115	231·3	1859	29·855	236·0
	1858	29·956	266·1	1861	29·544	224·5
	1860	29·919	230·0	...	...	...
	1862	29·897	234·7	...	...	...
	Average mortality,		240·5			230·2
December,	1857	29·989	215·7	1858	29·703	250·0
	1861	30·020	241·4	1859	29·651	263·4
	...	...	...	1860	29·709	257·7
	...	...	...	1862	29·767	259·0
	Average mortality,		228·5			257 5

Therefore, of the averages of the twelve months, six give a higher death-rate with a barometric reading above the average of the several years for those months, and on the other hand, six present a higher death-rate with the barometer below the average. It is more distinctly seen thus:—

Months which give a High Death-rate with a High Barometer.		Months which give a High Death-rate with a Low Barometer.	
February,	273·2	January,	276·9
April,	246·9	March,	253·3
July,	206·7	May,	226·3
August,	192·7	June,	217·0
September,	195·9	October,	200·9
November,	240·5	December,	257·5
Mean of Mortality,	225·99	Mean of Mortality,	238·65

Here again, then, it is plain that the accumulated mortality of those months in which there is a high death-rate with a low state of the barometer, is greater than the accumulated mortality of those months in which the death-rate and barometric reading are both above the average. It is noticeable that the average death-rate of those months in which there is a direct relationship between the barometer and the mortality, corresponds exactly with the mean death-rate of the whole term of six years, whilst the average death-rate of the months in which the relationship between the barometric reading and the mortality is inverse, is much higher.

Now, if I take the mean of the averages of all the months with a barometric reading above the mean of the six corresponding months, and the mean of the averages of all the months with a barometric reading below the mean of the six corresponding months, the result is the same, though not, of course, so distinctly shown:—

	Jan.	Feb.	March.	April.	May.	June.	Means.
Average mortality of the months with a mean height of barometer above the mean of the six corresponding months, ...	253·8	273·2	246·6	246·9	212·7	204·4	
Average mortality of the months with a mean height of barometer below the mean of the six corresponding months, ...	276·9	241·7	253·3	238·8	226·3	217·0	
	July.	**August.**	**Sept.**	**Oct.**	**Nov.**	**Dec.**	
Average mortality of the months with a mean height of barometer above the mean of the six corresponding months, ...	206·7	192·7	195·9	195·5	240·5	228·5	224·8
Average mortality of the months with a mean height of barometer below the mean of the six corresponding months, ...	202·4	182·7	171·3	200·9	230·2	257·5	225·0

Again: the mean height of the barometer for the whole period of six years is 29·843. Now, if I take this reading as a standard of reference, I find that of the seventy-two months thirty-eight afford a higher, and thirty-four a lower mean reading, and the result, in regard to the ratio of mortality from all causes, is the same as was observable when I employed the means of the corresponding months as standards of reference; namely, that the mortality is greatest with a low reading of the barometer: or more clearly thus:—

Average mortality of thirty-four months, in which the mean height of the barometer for each month was *below* 29·843, ...	228·14
Average mortality of thirty-eight months, in which the mean height of the barometer for each month was *above* 29·843, ...	223·52
Excess of mortality with a low barometer,	4·62

Again: if I arrange the months in seasons, representing winter by the months December, January, and February, and the other seasons accordingly, I find that of the eighteen winter months, the mean height of the barometer in *ten* was below, and in *eight* above 29·843: of the eighteen spring months the mean height of the barometer in *ten* was below, and in *eight* above 29·843: of the eighteen

summer months, the mean height of the barometer in *seven* was below, and in *eleven* above 29·843; and of the eighteen autumn months, the mean height of the barometer in *seven* was below, and in *eleven* above 29·843. The average mortality of these several groups of months is seen thus:—

WINTER.

Average mortality of ten winter months, each with a mean height of barometer	*below*	29·843,	258·6
,, ,, eight ,, ,,	*above*	...	254·8
Excess of mortality with low barometer,			3·8

SPRING.

.. .. ten spring months, each with a mean height of barometer	*below*	29·843,	238·7
.. .. eight ,, ,,	*above*	...	235·8
Excess of mortality with low barometer,			2·9

SUMMER.

.. .. seven summer months, each with a mean height of bar.	*below*	29·843,	200·97
.. .. eleven ,, ,,	*above*	...	200·76
Excess of mortality with low barometer,			0·21

AUTUMN.

.. .. eleven autumn months, each with a mean height of bar.	*above*	29·843,	214·5
.. .. seven ,, ,,	*below*	...	197·0
Excess of mortality with *high* barometer,			17·5

It would seem, then, that over the whole year, and in the seasons of winter, spring, and summer, the relationship between the mean monthly height of the barometer and the death-rate is *inverse;* but that in autumn it is *direct.* And I think that, although it is not a law without exceptions, there is an indication of a law, if the data be sufficient to determine it, of an increased mortality with a *low* barometer. But I must speak guardedly on this point, because the result of the inquiry differs from those obtained by many acute investigators; and I think it right to mention particularly that it is opposed to the opinion of talented investigators who have made similar observations in Berlin, Dresden, and Paris. And I may mention, moreover, that the results which are shown with reference to the influence of drought and humidity upon the death-rate from all causes are also opposed to those obtained by some of the same observers. At the close of the paper I shall present the principal data in a different form, in order to test still more rigidly the accuracy of these inductions.

The Range of the Barometer.—Intimately associated with the previous inquiry concerning the influence of the monthly mean height of the barometer upon the mortality, is that of the influence of the monthly range of the barometer. Referring to Table C, we find the following relations exhibited between the two in the several sections:—

Section.	Mortality.	Monthly Range of Barometer.
Maximum Section, ...	269·49	1·564
Major Section, ...	233·25	1·363
Minor Section, ...	214·26	1·163
Minimum Section, ...	185·85	0·961
Means, .	225·71	1·262

The following table is composed of three factitious years, constituted severally of months in which the monthly range of the barometer was greater than in any of the corresponding months, of months in which the range was less than in any other of the corresponding months, and of the mean monthly range of the six corresponding months of the different years.

Months.	Greatest Barometric Range.	Least Barometric Range.	Mean Barometric Range.	Mortality with High Range.	Mortality with Low Range.	Mean Mortality of the Six corresponding Months.
January, . . .	1·855	1·026	1·486	243·4	304·1	265·3
February, . . .	2·079	1·289	1·540	330·6	250·1	257·4
March, ...	2·021	1·270	1·593	250·0	232·0	249·8
April, ...	1·860	0·734	1·277	290·2	223·5	242·8
May, ...	1 339	0·498	0·956	210·4	193·5	219·5
June, ...	1·102	0·652	0·877	219·1	188·6	208·6
July, ...	1·083	0·745	0·865	181·3	214·8	204·5
August, . . .	1·167	0·661	0·942	178·7	224·1	189·4
September, . . .	1·172	0·993	1·082	165·0	195·9	187·7
October, . . .	1·872	1·201	1·417	204·4	173·1	198·2
November, . .	2·129	1·295	1·695	236·0	224·5	237·1
December, . . .	1·853	1·204	1·429	263·4	259·0	247·9
Means, . . .	1·627	0·964	1·263	231·04	223·60	225·68

Both of these tables direct us to the conclusion that the mortality from all causes bears a direct relation to the range of the barometer: the greater the range of the barometer the greater the death-rate, and *vice versa*. And this is another item in favour of the supposition that the mortality from all causes is greater with a low than with a high barometer, because the greatest range of the barometer is exhibited when the mean reading of the instrument is low. In the latter table four months oppose what has now been stated respecting the influence of the barometric range; they are January, July, August, and September;

and this they do whether the results of the highest and lowest ranges only be compared, or a comparison of all the six corresponding months in each case be instituted.

The Influence of Drought and Humidity.

We have already seen that, with reference to mean temperature, the relative amount of humidity in the atmosphere is important. We are now briefly to examine into the influence which the various hygrometric conditions of the atmosphere exert upon mortality from all causes independent of other meteorological elements. It should always be remembered, however, that the amount of moisture necessary to the saturation of a given quantity of air varies with temperature,—the warmer the atmosphere the greater the quantity of moisture requisite to saturate it, and *vice versa*. We may test the influence of the relative amount of moisture upon mortality from three points: *first*, with reference to the number of rainy days; *second*, with reference to the quantity of the rainfall in inches: and, *third*, with respect to the relative saturation of the atmosphere with moisture, as ascertained by means of Mr Glaisher's hygrometric Tables.

Number of Rainy Days.—A rainy day, it may be premised, does not necessarily mean one on which rain continued to fall throughout, nor even during many hours; a shower of rain is sufficient to constitute a rainy day. Nor has the term any reference to the amount of rain deposited in a given time; very little or very much rain may fall on a rainy day. Hence, we often find, that with ten rainy days in one month, there is a greater rainfall than in fifteen rainy days of another month.

In the several sections of Table C, the relative number of rainy days to the mortality from all causes is as follows:—

Section.	Mortality.	Number of Rainy Days.
Maximum Section, ...	269·49	15·1
Major Section, ...	233·25	13·7
Minor Section, ...	214·26	15·4
Minimum Section, ...	185·85	15·3
Means, ...	225·71	14·9

A Table consisting of three factitious years, constituted of months with the greatest number of, and months with the fewest rainy days, together with the means of all the corresponding months, shows the following result. Where two months are alike in the number of rainy days, the one in which most or least rain fell is taken:—

ERRATUM.

I regret that, owing to the circumstances under which the last of the Meteorological data were obtained, an error has crept into one of the tables, and from the table, somewhat into the text also. The Meteorological data for the quarter ending December 1862 were obtained from the Observatory, on slips of paper, from time to time, as they were reduced. I was obliged in this way to anticipate their publication, as the time for reading my paper was fixed for the 2d of February. Unfortunately, in constructing Table B, the slip of paper with the figures representing *the number of rainy days, the rainfall in inches*, and *the direction and force of the winds* for November 1862, was employed in the October column, the figures which ought to have been there finding their way into the November column. This error in Table B still exists.

The errors in the smaller tables of the text, arising from this transposition, are not of serious import; and indeed, if they had not occurred, the inferences drawn from the tables would generally have been only more apparent. The errors do not exist in Tables A and C, nor in the Diagrams; and in order to correct the errors of the smaller tables, it is only necessary to test the accuracy of the figures, by reference to one of the latter tables, in the few instances in which the rainfall and winds of October and November 1862 are quoted as illustrations.

	January.		February.		March.		April.		May.		June.	
Months with Most Rainy Days, . .	21	296·6	17	246·9	22	220·2	16	243·6	16	228·4	20	215·0
Months with Fewest Rainy Days, .	14	214·0	6	237·7	11	232·0	9	223·5	13	219·3	11	188·6
Means of the Six Corresponding Months,	16·6	265·3	12·1	257·4	16·1	249·8	12·2	242·8	13·0	219·5	13·8	208·6

	July.		August.		September.		October.		November.		December.	
Months with Most Rainy Days, . .	22	192·3	22	173·1	19	165·0	22	210·4	19	224·5	21	259·0
Months with Fewest Rainy Days, .	11	214·8	10	224·1	13	195·9	10	204·4	10	266·1	13	241·4
Means of the Six Corresponding Months,	16·5	204·5	16·0	189·4	15·3	187·7	15·7	198·2	15·2	237·1	16·2	247·9

In this Table the general indication is, that the mortality is greatest in those months which have the greatest number of rainy days. There are certain exceptions. March is the first month in which the general order is reversed, and in which, therefore, there is the highest mortality with the fewest rainy days. But if, instead of comparing only the months of the absolute highest and absolute lowest number of rainy days, we compare the four months which are below the mean of the six with the two that are above the mean, we find that the general rule holds good,—those months with the highest number of rainy days giving the average mortality 251·7, whilst those months with the fewest rainy days give an average mortality of only 249·1. The same may be said of July, where the average mortality of the three months with the greater number of rainy days is 206·9, whilst the average death-rate of the months with the fewest rainy days is only 202·2. The months of August, September, and November, which, in the above Table, oppose the general indication, continue so to do even when all the months are compared. Thus, the average mortality of the Augusts with most rainy days is only 177·9, whilst the average of those with the fewest rainy days is 200·7. Of September, in the same order, the proportion is 171·3 (most) to 195·9 (fewest). Of November, 229·7 (most) to 244·4 (fewest).

Quantity of Rain in Inches.

Table C. shows the following results:—

Section.	Mortality.	Rain in Inches.
Maximum Section, ...	269·49	3·18
Major Section, ...	233·25	3·06
Minor Section, ...	214·26	3·27
Minimum Section, ...	185·85	3·18
Means, ...	225·71	3·17

With the factitious years constituted as in the previous case, we have the following results:—

Months.	Most Rain.	Least Rain.	Mean Rainfall of the Six Corresponding Months.	Mortality with Most Rain.	Mortality with Least Rain.	Mean Mortality of the Six Months.
January, . .	5·32	2·77	3·82	296·6	253·2	265·3
February, . .	3·38	1·13	2·32	246·9	237·7	257·4
March, . . .	5·10	1·95	3·55	220·2	257·8	249·8
April, . . .	3·20	1·04	2·11	222·7	223·5	242·8
May, . . .	3·89	0·29	2·07	228·4	193·5	219·5
June, . . .	4·34	2·04	2·98	219·1	188·6	208·6
July, . . .	4·31	1·82	3·14	224 0	214·8	204·5
August, . .	6·44	2·27	3·39	173·1	178·7	189·4
September, . .	5·27	1·92	3·24	165·0	182·5	187·7
October, . .	5·14	2·36	4·41	210·4	208·0	198·2
November, .	6·63	2·38	3·37	224·5	266·1	237·1
December, . .	5·20	2·98	3·85	259·0	241·4	247·9
Means, .	5·82	1·91	3·18	224·16	220·49	225·7

In the latter Table we see the same indication of a tendency to increase of mortality with increase of rainfall, the exceptional months being, as in the case of the number of rainy days, March, July, August, September, and November, but in this case April also slightly opposes the general indication. It is observable that, in both instances, whether with an excess or deficit of the rainfall, the mortality is below that of the average of the six corresponding months taken over the whole year. Now, if instead of being content with the indications as given by a comparison of the month of absolute highest with the month of absolute lowest rainfall, we compare all the months which have a rainfall above the average rainfall of each group of corresponding months, with all the months which have a rainfall below the mean of the six corresponding months, we obtain the following result:—

	Average Mortality of those Months which have a Rainfall greater than the Mean of the Six corresponding Months.	Average Mortality of those Months which have a Rainfall less than the Mean of the Six corresponding Months.
January, ...	273·60	257·10
February, ...	268·53	246·36
March, ...	236·30	263·66 +
April, ...	238·76	246·93 +
May, ...	226·26	212·73
June, ...	217·05	204·40
July, ...	206·90	202·20
August, ...	184·75	198·60 +
September, ...	184·50	191·00 +
October, ...	201·23	195·16
November, ...	231·72	242·46 +
December, ...	255·56	240·16
Means, .	227·09	225·06

This Table still leads us to the same opinion,—namely, that the relationship existing between the amount of rainfall and the mortality from all causes is direct over the whole year, although it may be inverse as regards those months which in the latter Table are distinguished by the sign +.

Humidity.—It remains to be seen what may be the character of the relationship between the death-rate from all causes and the amount of moisture in the atmosphere, as indicated by the *humidity* column. The sections of Table C give us, with respect to humidity, the following results:—

Sections.	Mortality.	Humidity (Sat. = 100).
Maximum Section,	269·49	87
Major Section,	233·25	86
Minor Section,	214·26	84
Minimum Section,	185·85	84
Means,	225·71	85

In the foregoing Table we have an indication of a direct relationship between its two elements,—the greater the humidity the greater the mortality, although in the minor and minimum sections the figures representing the average amount of moisture are identical. In the following Table I have taken the average mortality of those of the six corresponding months whose humidity is above the mean of the six, and the average mortality of those months whose humidity is below the mean of the six. The results are seen thus:—

Average Mortality of those Months which have a Humidity above the Means of the Six corresponding Months.			Average Mortality of those Months which have a Humidity below the Means of the Six corresponding Months.		
January,	(Aver. Mean Temp. 36°·73)	236·86	January,	(Aver. Mean Temp. 38°·20)	293·86 +
February,	(„ „ 39°·85)	244·10	February,	(„ „ 34°·90)	284·15 +
March,		+ 253·36	March,		246·60
April,		+ 251·82	April,		224·90
May,		+ 224·70	May,		193·50
June,		+ 210·16	June,		207·06
July,		+ 208·87	July,		195·90
August,		+ 189·50	August,		189·10
September,		+ 188·55	September,		186·15
October,	(Aver. Mean Temp. 48°·30)	189·70	October,	(Aver. Mean Temp. 46°·00)	206·70 +
November,	„ „ 39°·97)	232·00	November,	(„ „ 39°·10)	242·20 +
December,		+ 249·70	December,		246·03
	Mean,	223·28		Mean,	226·346

Here, for the first time, we meet with an indication of a dry air, irrespective of temperature, being more fatal than a humid atmosphere. Nevertheless, eight of the months tend to a conclusion in conformity with that which the former Tables have suggested, four of the months only conducing to an opposite opinion. If we seek an explanation in the mean temperature of the several months which determine the result of the latter table, we find that, with the exception of

January, the average mean temperature of those months which show an increased mortality with deficient moisture is below the average mean temperature of the corresponding months whose humidity was greater. If this be worth anything, it tends to corroborate a previous suggestion that *dry cold* is more fatal than humid cold.

On the Influence of certain Winds.

We have no winds in this country which bear comparison, in point of intensity, with such as are the bane of other lands. Of Siroccos, Harmattans, Simooms, Samiels, Khamsins, or Solanos, we have none. But we have an east wind; and although it is by no means the prevalent wind of the country, nevertheless, from its harshness, and from its evil influence upon certain classes of disease, it has acquired a just notoriety.

Table C. furnishes us with the following comparisons. The figures represent the average number of days on which the different winds blew.

Section.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm or Variable.	Mortality.
Maximum, .	2·91	2·22	3·00	3·36	3·09	5·67	5·13	3·22	1·90	269·49
Major, . . .	2·15	2·00	3·17	3·14	2·80	5·97	5·44	2·86	2·58	233·25
Minor, . . .	1·90	2·02	3·03	2·62	2·86	6·72	6·22	3·08	2·12	214·26
Minimum, .	1·58	1·72	3·00	2·88	3·28	6·57	6·33	2·64	2·58	185·85
Means, . .	2·13	1·99	3·05	3·00	3·01	6·23	5·78	2·95	2·29	225·71

In the foregoing Table we have a general indication of a direct relationship between mortality from all causes and winds from a point between N. and E., and of an inverse relationship between mortality from all causes and winds from a point between S. and W. The due E. wind shows little or no predominance in any one section more than another. The S.E. wind shows a tendency to blow directly as the mortality, but not uniformly through all the four sections. The due S. wind, with the exception of the first section, blows inversely as the mortality. The N.W. wind affords no determinate relationship. With respect to the number of calm days, or days on which the wind was so light and changeable as to afford no fixed direction, all that can be said is, that there are fewest of such days in the maximum section.

As it would require too much space to compare the relative frequency of the different winds with the death-rate month by month, it must suffice to do so year by year. This is done in the following Table, in which the average mortality of the years with the greatest number of days of each wind is compared with the average mortality of the years with the fewest days of each wind:—

	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	C. or V.
Average Mortality of the Years in which the number of days of each Wind was above the Mean of the Six, ...	229·5	235·8	235·8	233·0	222·0	220·1	218·9	227·4	221·9
Average Mortality of the Years in which the number of days of each Wind was below the Mean of the Six, ...	223·7	220·6	220·6	218·3	227·4	236·7	232·4	223·8	229·4

If this be a true guide to the relationship existing between the prevalence of certain winds and mortality from all causes, then it is manifest that all winds between N.W. and S.E. (north about) are directly related to the death-rate, whilst those winds blowing from points between S.E. and W. (south about) have an inverse relationship. It would seem, moreover, that calms, or light shifting winds, are less frequent when the mortality is high than when it is low.

Force of Winds.—There is nothing in Table A. to lead us to a decision as to the influence of the relative force of wind upon mortality from all causes. The force is there given in lbs., and, according to the several sections, stands related to the death-rate in the following manner :—

Section.	Mortality.	Force of Wind in lbs.
Maximum, ...	269·49	1·654
Major, ...	233·25	1·630
Minor, ...	214·26	1·737
Minimum, ...	185·85	1·346
Means, ...	225·71	1·592

Two factitious years, constituted of the months which show the greatest wind force of the six, and those which show the least, respectively, afford the following comparison :—

	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
Mortality of each Month of the Greatest Wind Force, ...	243·4	330·6	232·0	222·7	219·3	215·0	192·3	173·1	177·7	210·4	224·5	215·7
Do. do. Least do.	253·2	251·3	256·7	223·5	193·5	196·4	214·8	168·4	195·9	204·4	230·0	241·4

From this table it is evident, that our data do not furnish us with determinate information regarding the influence of the pressure of the wind upon mortality. There can be no doubt, however, and perhaps, if the data prove anything at all, they prove this, that whatever be the exact pressure of the wind, it acts in a two-fold manner upon the death-rate, each separate influence compensating the other. If we have high winds, and stormy weather on that account, the death-rate will be increased not only by the fatal exhaustion caused to debilitated persons, but also by the casualties which invariably occur both afloat and on shore during gales of wind. On the other hand, when the pressure of wind is low, the atmosphere is stagnant, becomes loaded in many localities with pestilent effluvia, and sickness, usually of the zymotic class, prevails. A breeze of wind on land sweeps

out the hotbeds of disease, clearing them of the products of animal and vegetable putrefaction, and rendering them wholesome habitations for the human family; just as in another sphere of usefulness it turns up the surface of the wide ocean, preserving its waters from corruption, and imparting the very essence of life to its creatures.

Recapitulation.

Such is a brief sketch of the first part of the subject which at the outset I proposed to lay before the Society. I have before said, that this is by no means intended to be an exhaustive treatise, and I repeat that I have not even exhausted all the information which the tables and diagrams are capable of affording. In both there is still abundance of materials for further research, even concerning the influence of weather upon mortality from all causes of which alone I am now speaking, which may be thrown into a variety of shapes for the purpose of elucidating peculiar theories. I have done nothing more than examine the leading characteristics of the subject; and that without endeavouring either to propound or substantiate any theory whatever. I am neither disappointed that my results do not always coincide with those obtained by other investigators of cognate subjects, nor am I gratified with the idea of raising opposite views. In some points, the results of my inquiries harmonise with those of several observers in other countries, while in some they are diametrically opposed; but this does not at all imply a want of accuracy, either on their part or mine; it is quite competent for the results to differ, and yet each be right regarding the particular locality to which he refers. I have endeavoured to treat the investigation in the simplest manner, by merely putting questions to the collected facts, and leaving them to supply the answers; and in the present recapitulation I would have it distinctly understood, that I do not assert that these statements inviolably express the influence of weather upon mortality in the towns of Scotland; but I think it very probable that they do so generally. It will be a matter of no small interest to learn from the circumstances of the next six years whether the present suggestions are tenable or not. There does not appear to me to have been during the six years from which the facts are gathered any fluctuation of other external agencies worthy of particular attention. Sanitary improvements are occurring from time to time, and when they are such as to affect a large community, a diminution in the death-rate should, to a certain extent, be ascribed to them; the other matters of importance, as the price of food and the like, have already been given in a tabular form. It is probable, then, that in Scotland the death-rate from all causes is influenced by

A. *Temperature.*

1. Below 50° Fahr., the relationship existing between mean temperature and the death-rate from all causes is *inverse*—the lower the temperature the higher

the mortality; but above 50° the relationship becomes *direct*, the death-rate increasing with the temperature. The months during which the latter condition is observable, are probably July and August; but in Scotland the mean temperature does not often rise so high as to render it a cause of alarm.

2. Over the whole year the relationship between the monthly range of temperature and the death-rate is inverse; but during the months of July, August, and September, it is direct. A similar relationship exists between the mean daily range of temperature and the death-rate.

3. It is probable that *dry* cold is more fatal than *humid* cold.

4. Extremes of temperature are always fatal, but eminently so when long sustained.

B. *Atmospheric Pressure.*

1. The results afforded by comparison of the relative height of the barometer with the death-rate from all causes are conflicting, but there is probably a preponderance in favour of the supposition that the relationship between the two is inverse in the colder, and direct in the warmer months.

2. The relationship between the monthly range of the barometer and the death-rate is direct.

C. *Drought and Humidity.*

The relationship existing between humidity (irrespective of temperature) and mortality appears to be direct in the colder, and inverse in the warmer months.

D. *Winds.*

Winds blowing from a point between N.W. and S.E. (north about) attend a high death-rate. Winds blowing from a point between S.E. and W. (south about) occur more frequently during months in which the mortality from all causes is low.

If in the foregoing tables the difference between the mortality with this and with that kind of weather be often apparently trifling, it must be remembered that one or two deaths, more or less, per 100,000 living, is a matter for serious consideration. It must be remembered also, that in the mortality from all causes, there are many compensating influences at work which tend to reduce and soften what would otherwise appear as remarkably prominent features in the inquiry. As a single example of this, it is plain that if deaths from diarrhœa were excluded, the death-rate from all causes would be relatively higher in cold weather than it now appears to be, for deaths from diarrhœa being low in cold and high in warm weather, tend to equalise the surface over the whole rate of mortality.

II.—The Influence of Weather upon Mortality from Special Causes.

It must be quite obvious that to have repeated the foregoing inquiries with each class of disease, or still more so with each individual disease, would have carried me far beyond the limits of a single paper. The tables and diagrams together, however, are sufficient to enable any one desirous of information concerning the influence of weather upon mortality from any of the diseases there given, to comprehend readily enough the relationship which the several meteorological data bear to the death-rates. All that I can venture to do with the remainder of the collected facts will be to trace out the more apparent bonds of union between the weather and the several diseases; and I may mention, in passing, that the reason why I have added to the titles and diagrams a representation of the deaths from all *specified* causes, is simply to afford an opportunity of determining the proportion which the death-rate from any individual disease bears to the entire mortality; this could not have been ascertained by a reference to the death-rate from all causes, since that comprises many deaths from causes which are not stated, and which might therefore be attributable to any of the diseases in the Registrar-General's Schedule. Following the order in which the diseases are arranged in the tables, I shall confine my remarks either to the class of diseases or to one of the representative diseases, as seems to promise most interest.

A. *The Influence of Weather upon Mortality from Zymotic Diseases.*

This class of diseases may be dismissed with very few remarks; not because the inquiry is either uninteresting or unimportant, but because it is very complex, and would require more time than can at present be afforded to treat it as it deserves. I shall speak of zymotic diseases only as a class.

The influence of season in determining the death-rate from these disorders may be inferred from the following order of months, the ratio of deaths being that afforded by the averages of the six corresponding months as in Table C.

Month.	Mortality.	Month.	Mortality.
January,	67·0	September,	54·7
November,	65·8	July,	50·7
December,	64·7	April,	50·3
February,	60·3	August,	47·5
October,	59·0	May,	45·0
March,	55·3	June,	44·7
Mean,	55·42.		

Perhaps the questions of greatest interest with respect to zymotic diseases are those which refer to the mean temperature, mean height of the barometer, direction and force of wind, and the rainfall.

The relationship existing between the mean temperature of the month and its death-rate from zymotic diseases is seen in the following table, in which, out of the seventy-two months, those with the highest and those with the lowest death-rate from such causes are selected:—

	Jan.	Feb.	March.	April.	May.	June.	Average.
Mean temperature of the months in which the highest death-rate occurred, .	39·6	39·3	39·2	42·7	49·8	58·9	
Mean temperature of the months in which the lowest death-rate occurred, . .	39·3	35·8	39·5	45·4	49·5	57·2	
	July.	August.	Sept.	Oct.	Nov.	Dec.	
Mean temperature of the months in which the highest death-rate occurred, .	56·0	60·0	56·1	46·0	39·4	39·9	47·24
Mean temperature of the months in which the lowest death-rate occurred, . .	59·0	56·0	53·7	49·5	38·5	44·9	47·36

Hence it appears that the influence of temperature upon the death-rate caused by zymotic diseases as a class is variable, and that the above data afford no indication of any general law.

The following table is constructed upon the same principle as the former, and refers to the mean height of the barometer:—

	Jan.	Feb.	March.	April.	May.	June.	Average.
Mean height of the barometer for the months in which the highest death-rate occurred, ...	29·864	29·840	29·804	29·767	29·959	30·032	
Mean height of the barometer for the months in which the lowest death-rate occurred, ...	30·065	29·998	29·852	30·177	29·823	29·961	
	July.	August.	Sept.	Oct.	Nov.	Dec.	
Mean height of the barometer for the months in which the highest death-rate occurred, ...	29·887	30·014	29·882	29·784	29·956	29·703	29·874
Mean height of the barometer for the months in which the lowest death-rate occurred, ...	30·050	29·885	29·723	29·936	29·544	29·989	29·917

The preceding table scarcely points to any general law, unless it be this, that during the colder months of the year the relationship existing between the height of the barometer and the death-rate from zymotic diseases is inverse, whilst during the warmer months it is direct. The months July and November do not conform to such a law; it may be remarked, however, that the July with the high death-rate was much cooler than the other, whilst the November with the high death-rate was rather warmer than its associate.

The following table is arranged in the manner of the two previous tables, and refers to the amount of the rainfall in inches:—

	Jan.	Feb.	March.	April.	May.	June.	Average.
Rainfall of the months in which the highest death-rate occurred, . . .	4·21	1·54	2·94	2·38	1·66	2·36	
Rainfall of the months in which the lowest death-rate occurred, . . .	2·98	1·13	1·95	1·04	2·81	2·35	
	July.	August.	Sept.	Oct.	Nov.	Dec.	
Rainfall of the months in which the highest death-rate occurred, . . .	4·31	1·87	3·82	5·14	2·38	4·00	3·05
Rainfall of the months in which the lowest death-rate occurred, . . .	2·76	3·35	5·27	3·34	6·63	3·37	3·08

Eight of the months show a higher death-rate with a greater rain-fall, whilst the remaining four present a higher death-rate with a smaller rain-fall; nevertheless the average quantity of rain of both the years so constituted is almost identical.

As it would be tedious to reproduce in the text the relative prevalence of all the winds given in the tables, it may suffice to select one of them, and to infer from the fluctuations in that the relative frequency of others. The wind which blows on the greatest number of days in the year is that from the south-west; it will therefore be the one most suitable to our present purpose.

	Jan.	Feb.	March.	April.	May.	June.	Average.
Days of south-westerly wind in the months with the highest death-rate,	11·0	10·5	5·0	3·5	4·5	7·0	
Days of south-westerly wind in the months with the lowest death-rate,	10·0	3·5	6·5	3·0	6·5	6·0	
	July.	August.	Sept.	Oct.	Nov.	Dec.	
Days of south-westerly wind in the months with the highest death-rate,	6·0	5·5	6·0	8·0	3·1	8·9	6·6
Days of south-westerly wind in the months with the lowest death-rate,	7·0	7·0	6·0	8·0	7·0	15·0	7·12

In this table there appears no indication of a law of the wind affecting the death-rate; the averages of both the factitious years are somewhat above the average frequency of south-westerly winds over the six years. I find, on arranging the due east winds in the same manner, that the average of the twelve months with the highest death-rate is 3·46, whilst the average of the months with the lowest death-rate is 2·88; and that both of these averages are somewhat below the average frequency of due east winds over the six years, which is 3·5.

It will be interesting, in the last place, to ascertain whether the relative force

of the winds exerts any influence upon the death-rate from zymotic diseases. The following table is constructed like the former, the figures representing the force of the winds in pounds weight:—

	Jan.	Feb.	March.	April.	May.	June.	Average.
Pressure of the winds during the months with the greatest death-rate, . .	2·95	1·77	1·88	1·18	1·79	1·36	
Pressure of the winds during the months with the least death-rate, . . .	2·36	1·80	1·89	1·45	1·34	1·67	
	July.	August.	Sept.	Oct.	Nov.	Dec.	
Pressure of the winds during the months with the greatest death-rate, . .	1·11	1·04	1·38	2·34	1·09	1·80	1·64
Pressure of the winds during the months with the least death-rate, . . .	1·44	0·82	1·38	1·28	2·36	2·32	1·68

The average force of the winds over the six years is 1·59, and therefore in both of the extreme years of the above table the averages are rather high. It might have been supposed, that when the death-rate from zymotic diseases was higher, the relative pressure of the wind would have been low, indicating *calm* rather than *breezy* weather; but if the above table proves anything, it is the very opposite of that.

That the weather has to do with fluctuations in the death-rate from zymotic diseases no one will doubt, but the manner of its operation it is difficult to explain. In countries where marshes form an ample source of some of these diseases, the effects of temperature and humidity are obvious enough; but in this country, in which the germs of such diseases spring from unrecognised sources, the influence of the weather in bringing them hither, developing, and propagating them, is not so plain. I make no comments upon the foregoing facts; it was not to be expected that any very striking results would be deducible from a mere comparison of the meteorological data with the death-rate from a class of diseases comprehending so great a variety. In the tables I have given the death-rates from three diseases of the zymotic class—viz., Typhus, Scarlatina, and Diarrhœa—and it would have been interesting to have shown the effects of meteorological phenomena upon each of these, but that must be left to another opportunity.

B. *The Influence of Weather upon Mortality from Phthisis Pulmonalis.*

Instead of examining the influence of weather upon the tubercular class of diseases as a whole, it will perhaps be more profitable to confine my remarks to one, and that the most fatal, of such diseases.

The order of the months according to the death-rate from phthisis, from the highest to the lowest, following the means of the six corresponding months as in Table C, is as follows:—

April,	35·9	July,	30·3
March,	35·1	December,	28·1
May,	33·6	November,	26·7
June,	33·5	August,	26·0
February,	32·3	September,	23·1
January,	30·6	October,	22·9

The average monthly ratio of deaths from phthisis pulmonalis, per 100,000 living at all ages, in the eight larger towns of Scotland, during the six years 1857–62 inclusive, is exhibited in the following table, together with some of the meteorological characteristics of the corresponding periods :—

Year.	Mortality.	Mean Temp.	Mean of the Monthly Ranges of Temperature.	Mean of the Daily Ranges of Temperature.	Mean Height of Barometer.	Mean of the Monthly Ranges of Barometer.	Total Rainfall in Inches.	Total of Days of N., N.E., and E. Winds.
1860	32·4	44·5	31·3	11·8	29·785	1·444	37·88	105·0
1862	30·1	46·1	30·0	11·5	29·812	1·218	45·29	74·0
1858	29·5	46·6	45·8	12·8	29·916	1·294	33·91	74·0
1857	29·4	48·0	49·8	11·8	29·893	1·236	30·56	98·8
1861	29·1	46·9	30·9	11·6	29·838	1·099	45·07	83·0
1859	28·7	46·7	33·6	12·7	29·817	1·289	37·17	76·0
Average,	29·7	46·5	36·9	12·0	29·843	1·263	38·31	85·1

The following table is constructed upon the plan of those previously given with other subjects of investigation; the months are not those of the same years, but those of any year in which the highest or lowest death-rate from phthisis occurred.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Average.
Mean temperature with highest mortality,	36·3	34·0	38·4	41·5	50·2	53·0	58·0	54·4	53·4	46·0	39·4	34·0	44·9
Mean temperature with lowest mortality,	39·6	39·3	43·0	41·3	49·5	57·2	63·8	56·0	53·7	45·8	37·1	41·8	46·5
Monthly range of temperature with highest mortality,	37·4	35·0	27·0	32·2	35·5	27·5	46·5	27·1	32·9	29·6	33·3	35·5	33·3
Monthly range of temperature with lowest mortality,	27·1	56·0	33·2	39·0	54·7	32·3	26·2	26·4	27·7	43·8	33·3	22·7	35·2
Daily range of temperature with highest mortality,	8·2	10·7	11·5	14·9	15·0	13·3	14·1	12·8	14·1	10·9	9·7	8·7	12·0
Daily range of temperature with lowest mortality,	9·6	10·8	10·9	13·5	14·8	13·6	13·1	12·6	11·0	12·0	11·0	8·7	11·8
Mean height of barometer with highest mortality,	30·038	29·932	29·639	29·978	29·831	29·674	29·832	29·575	29·979	29·784	29·956	29·651	29·822
Mean height of barometer with lowest mortality,	29·864	29·840	29·707	29·751	29·823	29·961	29·735	29·885	29·723	29 667	29·897	29·767	29·802
Range of barometer with highest mortality,	1·026	2·079	1·931	1·860	1·228	1·102	0·850	1·080	0·993	1·261	1·872	1·858	1·428
Range of barometer with lowest mortality,	1·855	1·289	1·270	1·289	1·339	0·694	0·790	0·944	1·172	1·236	1·513	1·204	1·216
Rain in inches with highest mortality,	3·09	2·69	3·52	1·18	2·18	4·34	2·17	3·79	2·44	5·14	2·38	3·66	3·05
Rain in inches with lowest mortality,	4·21	1·54	4·18	3·20	2·81	2·35	3·87	3·35	5·27	4·60	6·32	5·20	3·91
Days of N., N.E., and E. winds with highest mortality,	6·0	8·0	6·0	13·0	8·0	11·0	3·0	6·0	6·0	4·0	10·0	8·0	7·42
Days of N., N.E., and E. winds with lowest mortality,	1·0	1·5	4·0	12·0	8·5	14·0	4·0	7·0	3·0	10·0	2·0	2·0	5·75

From the foregoing tables it would seem:—

1. That a low mean temperature of the winter months gives rise to an increase in the death-rate from phthisis, and that this relationship is the more clearly observable if the low temperature be sustained for some time without intermission, as in the case of the months from November 1859 to February 1860 inclusive. A high summer temperature does not seem to increase the fatality of phthsis. It is only when the temperature of winter is remarkably low that the increased death-rate from phthisis is distinctly traceable to that cause.

2. That the relationship between the monthly range of temperature and the death-rate from phthisis is uncertain, and that the latter is not under the control of the former.

3. That the daily range of temperature exerts no constant influence upon the death-rate from phthisis.

4. That there is no constant relationship observable between the mean monthly height of the barometer and the death-rate from phthisis.

5. That if there be any indication of a constant relationship between the monthly range of the barometer and the death-rate from phthisis, it is that the death-rate increases with the range.

6. That the rainfall bears no constant relationship to the death-rate from phthisis. It is possible, however, that it may be inverse in the colder and direct in the warmer months.

7. That possibly an increase in the number of days during which north, north-east, and east winds prevail, may give rise to an increase in the death-rate from phthisis.

C. *The Influence of Weather upon Mortality from Bronchitis.*

The influence of the weather upon diseases of the respiratory organs differs greatly from its influence upon phthisis pulmonalis. In the former, the atmosphere comes immediately into contact with the seat of the malady; in the latter, it merely touches a local manifestation of the disease. It is the oversight of this difference that leads to so much disappointment in the employment of change of climate as a remedial agent. In selecting a locality for the residence of a patient afflicted with a disease of the respiratory system,—as bronchitis, asthma, or the laryngeal affections,—too much care cannot be observed in matters meteorological; but, in choosing a winter resort for a phthisical patient, there are many circumstances of more weighty importance than the weather, to the consideration of which meteorology ought to be subordinated. Change of climate as a remedial agent in the treatment of consumption is exceedingly valuable when properly employed, but equally mischievous when used without a due regard to all the

circumstances of the case. To treat consumption by change of climate on meteorological grounds alone, is simply to endeavour to combat a symptom without reference to the pathology of the disease, and it would be quite as reasonable to expect a cure from the mere use of a poultice or a cough mixture. To dispatch a consumptive patient to a foreign country only for meteorological reasons, if he be unable to enjoy the change, in spite of the anxiety, fatigue, and discomforts which must attend the sacrifice of his ordinary pursuits, the separation from his friends and a sojourn amongst strangers, is not useless only, but *cruel*. But this is a digression from which, perhaps, it would be better that I should refrain.

The order of the months according to the death-rate from bronchitis, from the highest to the lowest, following the means of the six corresponding months as in Table C, is as follows:—

January,	30·4	May,	16·9
February,	30·0	June,	13·9
December,	26 9	October,	12·4
March,	25·2	July,	10·5
April,	22·8	September,	9·5
November,	22·7	August,	8·7
Mean,	19·16		

The average monthly ratio of deaths from bronchitis, per 100,000 living at all ages, in the eight larger towns of Scotland during the six years 1857–62 inclusive, is exhibited in the following table, together with some of the meteorological characteristics of the corresponding periods:—

Year.	Mortality.	Mean Temp.	Mean of the Monthly Ranges of Temperature.	Mean of the Daily Ranges of Temperature.	Mean Height of Barometer.	Mean of the Monthly Ranges of Barometer.	Total Rainfall in Inches.	Total of Days on which N., N.E., and E. Winds blew.
1860	26·6	44·5	31·3	11·8	29·785	1·444	37·88	105·0
1862	25·6	46·1	30·0	11·5	29·812	1·218	45 29	74·0
1861	22·9	46·9	30·9	11·6	29·838	1·099	45·07	83·0
1858	15·6	46·6	45·8	12·8	29·916	1·294	33·91	74·0
1857	12·4	48·0	49·8	11·8	29·893	1·236	30·56	98·8
1859	11·9	46·7	33·6	12·7	29·817	1·289	37·17	76·0

As in the case of bronchitis there is a very remarkable difference between the death-rate in 1860 and that in 1859, instead of constituting factitious years for comparison, I may in this instance contrast the year of the highest with the year of the lowest death-rate. This is done in the following table:—

	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Year.
Mortality from bronchitis in 1860,	36·0	62·0	41·2	43·9	26·8	17·2	14·2	9·3	8·9	11·7	21·7	26·0	26·6
Mortality from bronchitis in 1859,	17·6	19·8	14·8	13·3	10·0	6·8	6·0	5·9	6·8	6·0	6·8	28·8	11·9
Mean temperature in 1860, . .	35·5	34·0	38·4	41·5	50·2	53·0	57·3	54·4	50·2	46·0	39·1	34·1	44·5
Mean temperature in 1859, . .	39·6	40·5	43·0	41·3	51·9	56·6	59·0	57·8	52·3	45·8	39·4	34·0	46·7
Monthly range of temperature in 1860, ...	28·5	35·0	27·0	32·2	35·5	27·5	30·9	27·1	35·0	29·6	26·5	40·8	31·3
Monthly range of temperature in 1859, ...	27·1	29·0	33·2	39·0	42·4	35·3	32·4	30·7	28·2	43·8	26·9	35·5	33·6
Mean of the daily ranges of temperature in 1860, ...	8·1	10·7	11·5	14·9	15·0	13·3	14·5	12·8	13·8	10·9	8·1	8·1	11·8
Mean of the daily ranges of temperature of 1859, ...	9·6	9·8	10·9	13·5	19·7	16·1	14·3	14·7	13·6	12·0	10·4	8·7	12·7
Mean height of the barometer in 1860, ...	29·529	29·932	29·639	29·978	29·831	29·674	29·988	29·575	29·868	29·784	29·919	29·709	29·785
Mean height of the barometer in 1859, ...	29·864	29·709	29·707	29·751	30·046	29·934	30·050	29·850	29·722	29·667	29·855	29·651	29·817
Monthly range of the barometer in 1860, ...	1·810	2·079	1·931	1·860	1·228	1·102	0·745	1·080	1·165	1·261	1·641	1·426	1·444
Monthly range of the barometer in 1859, ...	1·855	1·375	1·270	1·289	0·498	0·653	1·083	1·167	1·063	1·236	2·129	1·853	1·289
Amount of rain in inches in 1860,	4·56	2·69	3·52	1·18	2·18	4·34	1·82	3·79	1·92	5·14	2·83	3·91	37·88
Amount of rain in inches in 1859,	4·21	3·38	4·18	3·20	0·29	2·04	2·76	2·27	3·21	4·60	3·37	3·66	37·17
No. of days on which N., N.E., and E. winds blew in 1860,	9·0	8·0	6·0	13·0	8·0	11·0	9·0	6·0	7·0	4·0	13·0	11·0	105·0
No. of days on which N., N.E., and E. winds blew in 1859,	1·0	2·0	4·0	12·0	11·0	11·0	7·0	1·0	3·0	10·0	6·0	8·0	76·0

If the foregoing tables are to be trusted, it would seem—

1. That there is an inverse relationship between temperature and the death-rate from bronchitis in all seasons, but that this is more remarkable in the winter months, and especially when the cold is severe and protracted.

2. That possibly there may be an inverse relationship between the monthly range of temperature and the death-rate from bronchitis over the whole year, but the relationship varies with the season.

3. That the relationship between the daily range of temperature and the death-rate from bronchitis also varies with the season; but there is no indication of any constant correspondence.

4. That possibly the relationship between the mean height of the barometer and the death-rate from bronchitis may be inverse in summer and direct during the remainder of the year. And that there is no constant relationship between the death-rate from bronchitis and the monthly range of the barometer.

5. That the rainfall does not influence the death-rate from bronchitis. To the last two suggestions it may be added, that although the tables do not indicate any constant correspondence between them, nevertheless it is highly probable that the state of the barometer, and the hygrometric condition of the atmosphere, do exert a powerful influence upon the mortality from bronchitis, and that the reason why such influence is not more distinctly visible is this, that whilst a dry atmosphere with a high barometer is prejudicial to one class of bronchitic patients it favours another, and *vice versa;* so that, the one class balancing the other, the influence is not discoverable upon the whole death-rate.

6. That the north, north-east, and east winds decidedly tend to increase the death-rate from bronchitis.

D. *The Influence of Weather upon Mortality from all Causes at different Ages.*

There are two periods in a lifetime at which the influence of the weather, equally with that of other external causes, is felt most keenly,—infancy and old age. In childhood, functional activity is bestowed upon the construction and development of the body; in old age, it is required to combat a natural tendency to decay; at both of these periods, the powers of resistance are feeble. Whatever causes interfere with the due performance of the vital functions, are more potent for mischief at these than at other periods of life. The adult can bear an amount of fatigue, both mental and physical, deprivations of food and sleep, and vicissitudes of weather, with far less danger to vitality than others can do at the extremes of life. A glance at the diagram in which the curves of mortality from all causes, at different ages are laid down, will show this distinctly. The curve representing the death-rate from 0–5 years fluctuates more than the rest; next to that in point of angularity is the death-rate of the aged. The curve of mortality in youth deviates but slightly, and that representing the death-rate in adult life would be much less prominent in its deviations, if it were seen in periods of twenty years each, as in the curves of youth and old age, supposing the latter to extend to eighty.

The order of the months, according to the death-rate from all causes at different periods of life, from the month with the highest to the month with the lowest death-rate, following the means of the six corresponding months, is as follows:—

0–5 Years.		5–20 Years.		20–60 Years.		60 Years and Upwards.	
January,	122·7	April,	25·1	January,	72·8	January,	45·9
February,	121·9	March,	24·3	March,	70·2	February,	44·0
December,	113·9	February,	24·1	December,	67·8	December,	43·5
March,	113·9	January,	23·9	February,	67·0	April,	42·1
November,	111·5	May,	23·8	April,	67·9	March,	41·5
April,	107·5	June,	23·3	May,	63·2	November,	41·3
May,	97·2	December,	22·7	November,	61·9	May,	35·2
October,	96·2	July,	22·5	June,	59·2	June,	34·9
July,	96·1	November,	22·4	July,	54·7	July,	31·2
September,	92·2	October,	21·5	August,	51·7	October,	28·8
June,	91·3	September,	20·3	October,	51·0	August,	28·7
August,	89·1	August,	19·7	September,	48·6	September,	26·7
Means,	104·5		22·7		61·3		36·9

It is noticeable, that the order of the months, according to the death-rate in infancy and old age, is very nearly the same as that according to the death-rate from bronchitis; whilst the order of the months, according to the death-rate be-

tween five and twenty years, is more like that observed with phthisis, the order of the months, according to the death-rate between twenty and sixty years, appearing to hover between the two.

The range of the death-rate between the month with the highest and the month with the lowest mortality varies with the period of life. As already adverted to, it is 33·6 in infancy, 19·2 in old age, and only 5·4 in youth. In adult life, over a period of forty years, the range is 24·2.

I shall not extend my paper by pursuing the same investigations with this as with the previous subjects of inquiry. There is only one more question I will ask of the facts now before me. It is this,—What is the effect of protracted cold in winter, and of sustained heat in summer, upon the death-rates at different periods of life? The question is answered in the following table. The periods selected for the inquiry are from November 1859 to April 1860, and from May to October 1857:—

	Nov.	Dec.	Jan.	Feb.	March.	April.
Mean Temperature in 1859–60	39·4	34·0	35·5	34·0	38·4	41·5
Average of the Six corresponding Months	39·5	38·8	37·4	38·2	39·8	43·2
Death-rate 0–5 years, 1859–60	107·8	114·4	131·2	155·2	131·2	130·4
Average, &c.	111·5	113·9	122·7	121·9	113·9	107·5
Death-rate 5–20 years, 1859–60	23·6	23·2	27·4	28·3	26·9	26·8
Average, &c.	22·4	22·7	23·9	24·1	24·3	25·1
Death-rate 20–60 years, 1859–60	63·4	77·3	79·1	86·1	76·2	77·5
Average, &c.	61·9	67·8	72·8	67·0	70·2	67·9
Death-rate 60 &c. years, 1859–60	41·5	49·0	42·8	60·7	48·4	55·2
Average, &c.	41·3	43·5	45·9	44·0	41·5	42·1

	May.	June.	July.	Aug.	Sept.	Oct.
Mean Temperature in 1857	49·8	57·4	58·0	60·0	56·1	49·6
Average, &c.	50·3	55·9	56·8	57·2	52·5	47·2
Death-rate 0–5 years, 1857	100·2	90·7	100·3	114·5	111·0	93 6
Average, &c.	97·2	91·3	96·1	89·1	92·2	96·2
Death-rate 5–20 years, 1857	22·6	25·1	20·9	19·4	19·1	22·9
Average, &c.	23·8	23·3	22·5	19·7	20·3	21·5
Death-rate 20–60 years, 1857	61·0	63·4	56·6	59·3	51·3	57·4
Average, &c.	63·2	59·2	54·7	51·7	48·6	51·0
Death-rate 60 &c. years, 1857	35·3	34·1	32·8	31·0	30·0	34·0
Average, &c.	35·2	34·9	31·2	28·7	26·7	28·8

From the foregoing table it would seem:—

1. That a protracted low temperature in winter largely increases the death-rate amongst children under five years of age; and that the death-rate rises almost immediately upon the fall of the thermometer, and falls again so soon as the temperature begins to rise.

2. That a continued low temperature perceptibly increases the death-rate amongst those between five and twenty years of age, though to a much less extent than in infancy; and the mortality curve does not rise so suddenly upon the fall in the curve of temperature.

3. That continued cold also raises the death-rate amongst adults, more perceptibly than in youth, but less than in infancy.

4. That in old age continued cold is prejudicial, but the death-rate does not rise so suddenly as either in infancy or in adult life.

5. It would appear from the foregoing remarks, that severe winter weather induces acute inflammatory diseases in infancy and adult life, rapidly cutting off its victims; that it increases the death-rate of the aged by aggravating chronic diseases; and that it probably cuts off only those in youth who are previously debilitated by some exhausting disease, as phthisis.

6. That a high temperature in summer, especially if long sustained, increases infantile mortality.

7. That such high summer temperature scarcely affects the death-rate in youth.

8. That it slightly increases the mortality in adult life.

9. And that it also slightly increases the death-rate of the aged.

10. That care ought to be taken to avoid exposure to the direct influence of the weather when the mean temperature sinks below 39° in winter, or rises above 57° in summer.

III.—General Resumé.

In the foregoing investigations I have employed as the standards of reference either the means of the six corresponding months, or the means afforded by the six years. In many instances I have drawn my conclusions from the comparison of two extreme years, factitiously constructed out of the whole term of seventy-two months. I have been urged to such modes of investigation for the sake of conciseness, having had to treat of a comprehensive subject within too narrow limits. In conclusion let me place the data before the Society in one other form, which will serve both as a check upon the previous inductions, and also as a *resumé* of the entire subject.

I shall employ as the standards of reference, in this instance, the means of the six corresponding *seasons*, and compare with them the *major* and *minor* readings, respectively, of the eighteen months comprised in the several periods from which the means are derived. It is unnecessary, or, at least, would occupy too much space, to repeat the minor meteorological details; I shall therefore restrict the present inquiry to the influence of mean temperature, the mean height of the barometer and the humidity as represented by the rainfall in inches.

Resumé: On the Influence of Mean Temperature.

Average monthly mean temperature of the six winters (Dec., Jan., Feb.),	.		38°·1
,, ,, ,,	springs (March, April, May),		44°·4
,, ,, ,,	summers (June, July, Aug.),		56°·6
,, ,, ,,	autumns (Sept., Oct., Nov.),		46°·4

In the following table the upper lines represent the average mortality of the months whose mean thermometric readings are severally *above* the standard of reference, whilst the lower lines represent the average mortality of the months with thermometric readings *below* the standard :—

	At all Ages from				From all Causes at			
	All Causes.	Zymotic Diseases.	Phthisis.	Bronchitis.	0–5 Years.	5–20 Years.	20–60 Years.	60, &c. Years.
Average mortality of *ten* winter months, with a mean temperature *above* 38°·1, ...	245·51	63·90	29·25	25·49	116·60	23·23	64·71	41·10
Average mortality of *eight* winter months, with a mean temperature *below* 38°·1, ...	271·11	64·12	31·74	33 69	123·17	24·04	74·64	48·76
Average mortality of *eight* spring months, with a mean temperature *above* 44°·4, ...	223·81	45·41	34·17	19·09	99·48	24·25	63·41	36·72
Average mortality of *ten* spring months, with a mean temperature *below* 44°·4, ...	248·35	54·44	35·46	23·74	111·58	24 53	70·02	42·01
Average mortality of *eleven* summer months, with a mean temperature *above* 56°·6, . .	201·40	49·09	29·43	9·70	94·73	20·96	54·57	31·13
Average mortality of *seven* summer months, with a mean temperature *below* 56°·6, . .	199·97	45·29	30·77	13·19	88·36	23·19	56·20	32·37
Average mortality of *nine* autumn months, with a mean temperature *above* 46°·4, ...	190·22	54·56	23·10	11·32	92·28	20·06	49·60	28·12
Average mortality of *nine* autumn months, with a mean temperature *below* 46°·4, ...	225·14	65·11	25·47	18·47	107·61	22·77	58·11	36·40

The foregoing table—

1. Corroborates the suggestion previously made, that the relationship between mortality from all causes and mean temperature is *inverse* when the mean temperature is below 50°, and *direct* when the temperature is higher. Probably the bad effects of a high mean temperature are not perceptible until it has been maintained for some time at or above 56°·6. In general terms, the relationship between mean temperature and mortality from all causes is inverse in winter, spring, and autumn, but direct in summer.

2. Seems to indicate a relationship between mean temperature and mortality from zymotic diseases similar to that between mean temperature and death from all causes; namely, *inverse* in winter, spring, and autumn, but *direct* in summer. The previous suggestion was, that the relationship was variable and uncertain.

3. Corroborates the suggestions previously made concerning the influence of mean temperature upon the death-rate from Phthisis Pulmonalis; namely, that a low winter temperature increases the mortality from phthisis, but only to a remarkable extent when the mean temperature is very low, and continuously so; and that a high summer temperature does not increase the fatality of phthisis.

It may be stated, generally, that the relationship between mean temperature and the death-rate from phthisis is *slightly inverse* all the year round.

4. Substantiates the previous statement that the relationship between mean temperature and the death-rate from bronchitis is *inverse* all the year round, and that such relationship is most distinct in winter.

5. Supports the previous statement, that the relationship between mean temperature and mortality from all causes is *inverse at all ages* in winter, spring, and autumn; and that such relationship is most distinct in infancy, and least so in youth. It also corroborates the statement that a high mean summer temperature increases infantile mortality; but it is opposed to the suggestion that a high mean summer temperature also increases the death-rate from all causes at other periods of life.

Resumé: On the Influence of the Mean Barometric Pressure.

Average monthly mean height of barometer of the six winters, ...	29·829
,, ,, ,, springs, ...	29·847
,, ,, ,, summers, ...	29·861
,, ,, ,, autumns, ...	29·837

In the following table, the upper lines represent the average mortality of the months whose mean barometric readings are severally *above* the standards of reference, whilst the lower lines represent the average mortality of the months with barometric readings *below* the standard :—

	At all Ages from				From all Causes at			
	All Causes.	Zymotic Diseases.	Phthisis.	Bronchitis.	0–5 Years.	5–20 Years.	20–60 Years.	60, &c. Years.
Average mortality of *nine* winter months, with mean height of barometer *above* 29·829, .	254·26	58·00	31·69	29·51	116·91	22·68	68·92	45·67
Average mortality of *nine* winter months, with mean height of barometer *below* 29·829, .	259·52	70·00	29·02	28·76	122·15	24·50	69·55	43·35
Average mortality of *eight* spring months, with mean height of barometer *above* 29·847, .	235·85	47·12	34·68	22·24	104·30	24·64	67·06	39·89
Average mortality of *ten* spring months, with mean height of barometer *below* 29·847, .	238·72	53·00	35·06	21·22	107·72	24·22	67·10	39·46
Average mortality of *ten* summer months, with mean height of barometer *above* 29·861, .	202·97	49·44	29·30	10·30	95·50	21·30	54·76	31·39
Average mortality of *eight* summer months, with mean height of barometer *below* 29·861, .	198·19	45·41	32·01	11·99	88·06	22·50	55·76	31·90
Average mortality of *eleven* autumn months, with mean height of barometer *above* 29·837, .	214·57	61·64	24·97	15·93	105·06	21·65	55·08	32·70
Average mortality of *seven* autumn months, with mean height of barometer *below* 29·837, .	196·86	57·00	23·20	13·27	91·90	21·04	51·91	31·57

The foregoing table—

1. Corroborates the previous suggestions, that over the whole year the relationship between the mean height of the barometer and the death-rate from all causes is *inverse*; that the relationship is *inverse* in winter and spring, and that it is *direct* in autumn; but it opposes the suggestion of an inverse relationship in summer.

2. Substantiates the suggestion, that the relationship between the mean height of the barometer and the death-rate from zymotic diseases is *inverse* in the colder, but *direct* in the warmer seasons.

3. Confirms the statement, that there is no constant relationship observable between the mean monthly height of the barometer and the death-rate from phthisis, the results both of the two colder and of the two warmer seasons being opposed to one another.

4. Suggests a *direct* relationship between the mean height of the barometer and the death-rate from bronchitis in winter, spring, and autumn; but an *inverse* relationship in summer,—a suggestion which, although not without conflict, is supported by the results previously obtained.

5. Shows that the influence of barometric pressure upon the death-rate from all causes varies with age. In infancy the relationship between the mean height of the barometer and the death-rate from all causes is probably inverse in winter and spring, but direct in summer and autumn: in youth the relationship is inconstant; in adult life it is also uncertain; in old age it is possibly nearly the opposite of that which obtains in infancy.

Resumé: On the Influence of Drought and Humidity.

Average monthly rainfall of the six winters,	. . . . .	3·33	inches.
„ „ „ springs,	. . . . .	2·58	„
„ „ „ summers,	. . . . .	3·17	„
„ „ „ autumns,	. . . . .	3·67	„

In the following table the upper lines represent the average mortality of the months whose mean rainfall is severally *above* the standard of reference, whilst the lower lines represent the average mortality of the months with a rainfall below the standard:—

	At all Ages from				From all Causes at			
	All Causes.	Zymotic Diseases.	Phthisis.	Bronchitis.	0-5 Years.	5-20 Years.	20-60 Years.	60, &c. Years.
Average mortality of *nine* winter months, with a rainfall *above* 3·33 inches, ...	257·06	69·44	29·20	27·48	120·37	24·12	69·21	43·66
Average mortality of *nine* winter months, with a rainfall *below* 3·33 inches, ...	256·72	58·55	31·52	30·80	118·68	23·06	69·26	45·36
Average mortality of *nine* spring months, with a rainfall *above* 2·58 inches, ...	239·29	52·80	34·33	22·24	108·97	24·42	66·18	39·42
Average mortality of *nine* spring months, with a rainfall *below* 2·58 inches, ...	235·60	48·00	35·44	21·10	103·43	24·40	67·99	39·88
Average mortality of *eight* summer months, with a rainfall *above* 3·17 inches, ...	198·60	45·12	30·24	13·42	88·55	22·84	55·61	31·51
Average mortality of *ten* summer months, with a rainfall *below* 3·17 inches, ...	202·65	49·60	29·72	9·15	95·11	21·02	54·88	31·69
Average mortality of *seven* autumn months, with a rainfall *above* 3·67 inches, . . .	205·53	60·30	23·36	15·27	100·13	20·77	52·90	31·47
Average mortality of *eleven* autumn months, with a rainfall *below* 3·67 inches, . . .	209·05	59·55	24·88	14·66	99·83	21·82	54·45	32·76

The foregoing table—

1. Suggests a direct relationship between the rainfall and the death-rate from all causes in winter and spring; but an inverse relationship in summer and autumn,—a suggestion which the previous results tend to support.

2. Suggests a direct relationship between the rainfall and the death-rate from zymotic diseases in winter, spring, and autumn; but an inverse relationship in summer. This suggestion also is nearly supported by the previous results.

3. Shows that there is a slight indication of an inverse relationship between the rainfall and mortality from phthisis in winter, spring, and autumn; but of a direct relationship in summer. These indications, however, are not very distinct, and are not well substantiated by the previous results.

4. Indicates a direct relationship between the death-rate from bronchitis and the rainfall in spring, summer, and autumn; but an inverse relationship in winter. This is not distinctly observable in the previous results; and probably the note appended to the fifth suggestion, under bronchitis, is the true explanation of the inconstancy of the relationship.

5. Shows that the influence of the rainfall upon the death-rate from all causes varies with the periods of life. In infancy the relationship appears to be direct in winter and spring, doubtful in autumn, and inverse in summer. At other periods of life the relationship is scarcely perceptible.

Here I must close my paper. I offer it as a slight contribution to medical climatology, and I trust there will be found in it something worthy of the consideration of those who take an interest in that still very obscure science. The interpretations that I have given of the facts at my disposal are such as I think they will bear without straining; but I place both facts and comments before the Society, so that any one who is interested in the matter may judge for himself.

TABLE A. IN WHICH THE MONTHS, WITH THEIR METEOROLOGICAL READINGS AND DEDUCTIONS, ARE ARRANGED IN THE ORDER OF THE RATIO OF MORTALITY FROM ALL CAUSES, THE HIGHEST RATIO BEING AT THE TOP AND THE LOW

IS DIVIDED INTO FOUR SECTIONS OF EIGHTEEN MONTHS, CORRESPONDING TO A MAXIMUM, MAJOR, MINOR, AND MINIMUM RATIO OF MORTALITY; THE MEANS OF EACH SECTION ARE GIVEN IN THE SECOND, AND THE MEANS O

	Month.	Ratio per 100,000 living of Deaths from all causes.	Mean of each Section.	Mean of the whole.	Mean Temperature.	Mean of each Section.	Mean of the whole.	Mean of the highest Temperatures.	Mean of each Section.	Mean of the whole.	Mean of the lowest Temperatures.	Mean of each Section.	Mean of the whole.	Monthly Range of Temperature.	Mean of each Section.	Mean of the whole.	Mean of the Daily Range of Temperature.	Mean of each Section.	Mean of the whole.	Mean Height of Barometer at Sea level, and 32°.	Mean of each Section.	Mean of the whole.	Monthly Range of Barometer.	Mean of each Section.	Mean of the whole.	No. of Rainy Days.	Mean of each Section.	Mean of the whole.	Quantity of Rain in inches.	Mean of each Section.	Mean of the whole.	Humidity. Sat. = 100.	Mean of each Section.	Mean of the whole.
1860	Feb.	330·6			34·0			39·2			28·6			35·0			10·7			29·932			2·079			13			2·69			85		
1847	Jan.	304·1			36·3			40·4			32·9			37·4			8·2			30·038			1·026			14			3·09			90		
1862	Jan.	296·6			38·4			42·0			34·8			21·7			7·1			29·686			1·451			21			5·52			90		
1860	April	290·2			41·5			48·9			34·0			32·2			14·9			29·978			1·861			10			1·18			82		
1860	March	283·2			38·4			44·0			32·5			27·0			11·5			29·639			1·931			19			3·52			86		
1860	Jan.	280·8			35·5			39·6			31·4			28·5			8·1			29·529			1·810			18			4·56			89		
1855	Nov.	266·1			39·4			44·4			34·4			33·3			9·7			29·956			1·872			10			2·38			89		
1859	Dec.	263·4			34·0			38·3			29·7			35·5			8·7			29·651			1·853			15			3·66			88		
1862	Dec.	259·0			41 8			46·2			37·4			22·7			8·7			29·767			1·204			21			5·20			89		
1856	March	257·8	269·49		39·5	38·3		45·5	43·1		33·6	33·4		56·0	36·4		11·9	9·8		29·852	29·800		1·587	1·564		13	15·1		1·95	3·18		85	87	
1860	Dec.	257·7			34·1			37·9			30·0			40·8			8·1			29·709			1·426			17			3·91			90		
1862	March	256·7			37·8			42·5			33·2			34·8			9·3			29·698			1·322			16			3·63			88		
1857	Jan.	253·2			35·7			39·8			31·6			50·1			8·2			29·698			1·534			16			2·77			87		
1862	Feb.	251·3			40·1			44·4			35·8			31·2			8·6			30·052			1·402			11			1·88			89		
1857	Feb.	250·1			39·3			44·3			34·3			56·0			10·8			29·840			1·289			11			1·54			89		
1862	April	250·0			44·6			51·5			37·8			39·7			13·7			29·881			1·204			14			2·99			83		
1857	March	250·0			39·2			44·0			34·5			49·5			9·5			29·804			2·021			16			2·94			86		
1856	Dec.	250·0			39·9			44·0			35·7			25·2			8·3			29·703			1·297			17			4·00			90		
1859	Feb.	246·9			40·5			45·4			35·5			29·0			9·8			29·709			1·375			17			3·38			88		
1857	April	243·6			42·7			48·7			36·7			46·0			12·0			29·769			1·142			16			2·38			84		
1859	Jan.	243·4			39·6			44·4			34·8			27·1			9·6			29·864			1·855			17			4·21			87		
1861	Dec.	241·4			38·0			42·8			33·3			30·6			9·5			30·020			1·459			13			2·98			90		
1860	May	240·0			50·2			57·7			42·6			35·5			15·0			29·831			1·228			15			2·18			81		
1856	Feb.	237·7			35·8			41·0			30·5			49·0			10·7			29·998			1·427			6			1·13			83		
1859	Nov.	236·0			39·4			44·5			34·2			26·9			10·4			29·855			2·129			14			3·37			89		
1862	Nov.	234·7			37·1			42·6			31·6			33·3			11·0			29·898			1·513			10			2·10			90		
1859	March	232·0	233·25		43·0	43·1		48·5	50·0		37·6	38·2		33·2	37·2		10·9	11 6		29·707	29·868		1·270	1·363		11	13·7		4·18	3·06		85	86	
1857	Nov.	231·3			43·7			48·3			39·2			50·0			9·1			30·115			1·719			12			2·89			90		
1860	Nov.	230·0			39·1			51·4			40·5			26·5			8·1			29·919			1·641			17			2·83			90		
1862	May	228·4			51·1			58·1			44·0			31·2			14·1			29·810			0·845			16			3·89			84		
1861	Feb.	228·1			39·5			44·1			34·9			29·7			9·3			29·681			1·670			15			3·32			88		
1858	April	227·1			43·8			51·4			36·1			61·0			15·3			29·942			1·434			9			1·86			78		
1861	May	225·4			49·1			56·9			41·3			39·0			15·6			30·070			0·937			14			1·57			80		
1861	Nov.	224·5			38·5			43·9			33·0			33·4			10·9			29·544			1·295			18			6·63			87		
1857	Aug.	224·1			60·0			67·1			53·0			53·0			14·1			30·014			0·661			10			1·87			85		
1858	July	224·0			56·0			63·4			48·7			44·0			14·7			29·887			0·945			17			4·31			83		
1861	April	223.5		225.71	45·4		46·3	52·4		52·6	38·3		40·5	34·0		36·8	14·1		12·0	30·177		29·843	0·734		1·262	9		14·9	1·04		3·17	83		85
1859	April	222·7			41·3			48·7			35·2			39·0			13·5			29·751			1·289			15			3·20			80		
1861	March	220·2			41·1			46·9			35·5			24·2			11·4			29·507			1·430			22			5·10			88		
1857	May	219·3			49·8			57·0			42·6			47·0			14·4			29·959			0·890			13			1·66			80		
1858	June	219·3			58·9			67·1			50·7			47·0			16·4			30·032			0·702			12			2·36			78		
1860	June	219·1			53·0			59·7			46·5			27·5			13·3			29·674			1·102			18			4·34			85		
1857	Dec.	215·7			44·9			49·2			40·7			38·0			8·5			29·989			1·338			14			2·37			88		
1862	June	215·0			52·4			58·7			46·0			27·3			12·6			29·733			1·074			20			3·99			83		
1860	July	214·8			57·3			64·5			50·1			30·9			14·4			29·985			0·745			11			1·82			83		
1856	Jan.	214·0	214·26		39·3	49·5		44·0	56·1		34·6	43·1		40·0	39·6		9·4	13·0		30·065	29·862		1·241	1·163		14	15·4		2·98	3·27		86	84	
1857	June	213·3			57·4			66·0			48·8			66·5			17·2			30·020			1·039			9			2·79			81		
1857	Sept.	210·8			56·1			62·5			49·7			47·9			12·8			29·882			1·104			15			3·82			87		
1857	July	210·5			58·0			65·1			51·0			46·5			14·1			29·832			0·850			15			2·17			80		
1858	May	210·4			49·5			56·9			42·1			54·7			14·8			29·823			1·339			16			2·81			81		
1860	Oct.	210·4			46·0			51·4			40·5			29·6			10·9			29·784			1·261			22			5·14			87		
1857	Oct.	208·0			49·6			55·3			43·9			47·0			11·4			29·803			1·246			16			2·36			90		
1858	Oct.	205·3			44·9			51·0			39·2			32·9			12·4			29·892			1·687			18			4·72			87		
1862	Oct.	204·4			47·1			53·4			40·7			32·8			12·7			29·620			1·872			19			6·32			88		
1861	July	204·4			56·8			63·6			50·0			27·6			13·6			29·619			0·777			20			3·94			84		
1858	Aug.	199·5			57·9			65·9			49·9			52·0			16·0			29·943			1·003			15			2·61			81		
1861	June	196·4			57·2			64·0			50·4			32·3			13·6			29·961			0·694			13			2·35			84		
1862	Sept.	195·9			53·4			60·3			46·4			32·9			14·1			29·979			0·993			13			2·44			88		
1858	Sept.	194·6			54·5			61·8			47·2			55·0			14·6			29·898			0·997			14			2·80			84		
1859	May	193·5			51·9			61·7			42·1			42·4			19·7			30·046			0·498			4			0·29			73		
1860	Aug.	192·4			54·4			60·8			48·0			27·1			12·8			29·575			1·080			19			3·79			85		
1862	July	192·3			53·8			60·4			47·3			26·2			13·1			29·735			0·790			22			3·87			83		
1859	June	188·6			56·6			64·7			48·6			35·3			16·1			29·934			0·652			11			2·04			78		
1859	Oct.	188·0	185·85		45·8	54·3		51·8	61·2		39·8	47·4		43·8	33·9		12·0	13·8		29·667	29·842		1·236	0·961		14	15·3		4·60	3·18		89	84	
1860	Sept.	182·5			50.2			57·1			43·3			35·0			13·8			29·868			1·165			14			1·92			87		
1859	July	181·3			59·0			66·2			51·9			32·4			14·3			30·050			1·083			14			2·76			81		
1859	Aug.	178·7			57·8			65·2			50·4			30·7			14·7			29·850			1·167			14			2·27			80		
1859	Sept.	177·7			52·3			59·2			45·5			28·2			13·6			29·722			1·063			17			3·21			82		
1861	Aug.	173·1			57·4			62·8			51·9			25·1			11·0			29·774			0·797			22			6·44			86		
1861	Oct.	173·1			49·5			55·4			43·7			30·9			11·6			29·936			1·201			14			3·34			90		
1862	Aug.	168·4			56·0			62·3			49·7			26·4			12·6			29·885			0·944			16			3·35			87		
1861	Sept.	165·0			53·7			59·2			48·2			27·7			11·0			29·723			1·172			19			5·27			88		

WINDS.																																	
N.	Mean of each Sect.	Mean of the whole.	NE.	Mean of each Sect.	Mean of the whole.	E.	Mean of each Sect.	Mean of the whole.	SE.	Mean of each Sect.	Mean of the whole.	S.	Mean of each Sect.	Mean of the whole.	SW.	Mean of each Sect.	Mean of the whole.
5·0			2·0			1·0			1·0			2·0			4·0		
2·0			1·0			3·0			3·0			4·0			8·0		
2·0			1·0			2·0			5·0			5·0			6·0		
4·0			4·0			5·0			3·0			2·0			3·0		
3·0			2·0			1·0			2·0			3·0			6·0		
3·0			3·0			3·0			4·0			4·0			5·0		
3·1			2·6			4·3			3·9			2·4			3·1		
4·0			2·0			2·0			3·0			3·0			6·0		
1·0			0·0			1·0			3·0			3·0			9·0		
3·0	2·91		2·5	2·22		1·5	3·00		0·5	3·36		0·5	3·09		6·5	5·67	
4·0			3·0			4·0			7·0			3·0			2·0		
3·0			6·0			9·0			4·0			2·0			3·0		
3·5			3·5			2·0			1·5			2·0			6·0		
2·0			2·0			4·0			6·0			3·0			3·0		
5·0			0·0			1·0			2·5			5·0			10·5		
3·0			2·0			2·0			3·0			4·0			7·0		
1·0			3·0			7·0			4·0			3·0			5·0		
0·8			0·4			1·2			4·1			4·8			8·9		
1·0			0·0			1·0			2·0			3·0			8·0		
2·0			3·0			7·0			4·0			3·0			3·5		
1·0			0·0			0·0			1·0			3·0			11·0		
3·0			2·0			1·0			2·0			3·0			7·0		
1·0			2·0			5·0			4·0			4·0			5·0		
2·0			2·5			4·5			6·5			2·5			3·5		
3·0			1·0			2·0			3·0			3·0			6·0		
2·0	2·15		1·0	2·00		1·0	3·17		1·0	3·14		2·0	2·80		8·0	5·97	
2·5			3·5			4·5			3·5			2·0			5·0		
2·0			3·0			8·0			6·0			2·0			2·0		
1·0			2·0			3·0			4·0			5·0			6·0		
2·0			2·0			2·0			4·0			3·0			7·0		
2·0			2·0			5·0			4·0			2·0			5·0		
4·0			3·0			3·0			1·0			1·0			6·0		
2·3			3·0			3·5			3·5			3·0			5·5		
2·0			2·0			2·5			2·0			2·0			6·0		
3·0		2.13	3·0		1·99	5·0		3·05	3·0		3·00	2·0		3·01	3·0		6·23
4·0			3·0			5·0			2·0			1·0			3·0		
1·0			2·0			1·0			2·0			3·0			8·0		
1·5			4·0			8·5			4·5			3·0			4·5		
0·5			1·0			2·0			3·0			4·0			7·0		
2·0			3·0			6·0			5·0			4·0			4·0		
0·5			0·0			1·0			3·0			3·0			15·0		
3·0			1·0			2·0			2·0			3·0			6·0		
2·0			2·0			4·0			3·0			3·0			3·0		
1·5	1·90		0·5	2·02		0·5	3·03		2·5	2·62		3·0	2·86		10·0	6·72	
2·0			4·0			6·0			4·0			3·0			4·0		
1·5			3·5			3·5			2·0			3·5			6·0		
1·5			1·0			0·5			2·0			2·0			9·5		
2·5			3·0			3·0			3·0			2·5			6·5		
2·0			1·0			1·0			2·0			4·0			8·0		
1·5			3·0			3·5			3·5			3·0			7·0		
2·2			2·8			2·1			1·2			1·5			7·5		
1·0			0·0			1·0			3·0			3·0			9·0		
1·0			2·0			3·0			3·0			4·0			7·0		
2·0			1·5			2·0			3·5			3·5			6·5		
3·0			4·0			7·0			4·0			3·0			3·5		
1·0			2·0			3·0			4·0			3·0			6·0		
2·0			0·5			0·5			2·5			2·5			10·0		
2·0			0·5			1·0			3·0			3·0			4·0		
2·0			2·0			2·0			3·0			4·0			4·0		
2·0			1·0			2·0			3·0			3·0			5·0		
2·0			1·0			2·0			3·0			3·0			7·0		
2·0			1·0			2·0			3·0			3·0			7·0		
2·0			1·0			2·0			3·0			4·0			6·0		
2·0			1·0			2·0			3·0			3

HEIR METEOROLOGICAL READINGS AND DEDUCTIONS, ARE ARRANGED IN THE ORDER OF THE RATIO OF MORTALITY FROM ALL CAUSES, THE HIGHEST RATIO BEING AT THE TOP AND THE LOWEST AT THE FOOT OF THE TABLE; EACH COLUMN EIGHTEEN MONTHS, CORRESPONDING TO A MAXIMUM, MAJOR, MINOR, AND MINIMUM RATIO OF MORTALITY; THE MEANS OF EACH SECTION ARE GIVEN IN THE SECOND, AND THE MEANS OF THE WHOLE IN THE THIRD COLUMNS.

| an f ch e- n. | Mean of the whole. | Mean of the lowest Tem- peratures. | Mean of each Sec- tion. | Mean of the whole. | Monthly Range of Tem- perature. | Mean of each Sec- tion. | Mean of the whole. | Mean of the Daily Range of Tempe- rature. | Mean of each Sec- tion. | Mean of the whole | Mean Height of Barom- eter at Sea level, and 32°. | Mean of each Section. | Mean of the whole. | Monthly Range of Barom- eter. | Mean of each Section. | Mean of the whole. | No. of Rainy Days. | Mean of each Sec- tion. | Mean of the whole. | Quan- tity of Rain in inches. | Mean of each Sec- tion. | Mean of the whole. | Humi- dity. Sat. =100. | Mean of each Sec- tion. | Mean of the whole. | WINDS. | | |

TABLE B. Representing the Meteorology and the Death-rate (from all and several Causes at all Ages, and from all Causes at several Ages), in the consecutive or

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.			Jan.	Feb.	March.	April.	May.	June.	July.	Au
	35·7	39·3	39·2	42·7	49·8	57·4	58·0	60·0	56·1	49·6	43·7	44·9	Mean temperature,	39·6	40·5	43·0	41·3	51·9	56·65	59·0	57·8	52·3	45·8	39·4	34·0			36·3	39·5	41·1	45·4	49·1	57·2	56·8	57·
	39·8	44·3	44·0	48·7	57·0	66·0	65·1	67·1	62·5	55·3	48·3	49·2	Mean of the highest temperatures,	44·4	45·4	48·5	48·7	61·7	64·7	66·2	65·2	59·2	51 8	44·5	38·3			40·4	44·1	46·9	52·4	56·9	64·0	63·6	62·
	31·6	34·3	34·5	36·7	42·6	48·8	51·0	53·0	49·7	43·9	39·2	40·7	Mean of the lowest temperatures,	34·8	35·5	37·6	35·2	42·1	48·6	51·9	50·4	45·5	39·8	34·2	29·7			32·9	34·9	35·5	38·3	41·3	50·4	50·0	51·
	50·1	56 0	49·5	46·0	47·0	66·5	46·5	53·0	47·9	47 0	50·0	38·0	Monthly range of temperature, ...	27·1	29·0	33·2	39·0	42·4	35·3	32·4	30·7	28·2	43·8	26·9	35·5			37·4	29·7	24·2	34·0	39·0	32·3	27·6	25·
	8·2	10·8	9·5	12·0	14·4	17·2	14·1	14·1	12·8	11·4	9·1	8·5	Mean daily range of temperature,	9 6	9·8	10·9	13·5	19·7	16·1	14·3	14·7	13·6	12·0	10·4	8·7			8·2	9·3	11·4	14·1	15·6	13·6	13·6	11·
	29·698	29·840	29·804	29·767	29·959	30·020	29·832	30·014	29·882	29 803	30·115	29·989	Mean ht. of barom.: sea-level & 32°	29·864	29·709	29·707	29·751	30·046	29·934	30·050	29·850	29·722	29·667	29·855	29·651			30·038	29·681	29·507	30·177	30·070	29·961	29·619	29·
	1·534	1·289	2·021	1·142	0·890	1·039	0·850	0·661	1·101	1·246	1·719	1·338	Monthly range of barometer, ...	1·855	1·375	1·270	1·289	0·498	0·653	1·088	1·167	1·063	1·236	2·129	1·853			1·026	1·670	1·430	0·734	0·937	0·694	0·777	0·
	16	11	16	16	13	9	15	10	15	16	12	14	Number of rainy days,.....	17	17	11	15	4	11	14	14	17	14	14	15			14	15	22	.9	14	13	20	2
	2·77	1·54	2·94	2·38	1·66	2·79	2·17	1·87	3·82	2·36	2·89	3·37	Rain in inches, ...	4·21	3·38	4·18	3·20	0·29	2·04	2·76	2·27	3·21	4·60	3·37	3·66			3·09	3·32	5·10	1·04	1·57	2·35	3·94	6·
	87	89	86	84	80	81	80	85	87	90	90	88	Humidity (Sat. = 100), ...	87	88	85	80	73	78	81	80	82	89	89	88			90	88	86	83	80	84	84	8
	3·5	0·5	1·0	2·0	1·5	2·0	1·5	2·35	1·5	1·5	2·5	0·5	N.	1·0	1·0	2·0	4·0	2·0	2·0	1·0	0·0	0·0	3·0	3·0	4·0			2·0	2·0	1·0	3·0	4·0	3·0	1·0	1·
	3·5	0 0	3·0	3·0	4·0	4·0	1·0	3·0	3·5	3·0	3·5	0·0	N.E.	0·0	0·0	1·0	3·0	3·0	4·0	2·0	0·0	1·0	3·0	1·0	2·0			1·0	2·0	1·0	3·0	3·0	4·0	2·0	0·
	2·0	1·0	7·0	7·0	8·5	6·0	0·5	3·5	3·5	3·5	4·5	0·0	E.	0·0	1·0	1·0	5·0	6·0	5·0	4·0	1·0	2·0	4·0	2·0	2·0			3·0	2·0	1·0	5·0	3·0	7·0	3·0	1·
	1·5	2·5	4·0	4·0	4·5	4·0	0·5	3·5	2·0	3·5	3·5	1·0	S.E.	1·0	2·0	1·0	2·0	5·0	3·0	2·0	1·0	3·0	4·0	3·0	3·0			3·0	4·0	2·0	3·0	1·0	4·0	3·0	1·
	2·0	5·0	3·0	3·0	3·0	3·0	2·0	3·0	3·5	3·0	2·0	3·0	S.	3·0	3·0	2·0	1·0	4·0	2·0	2·0	5·0	4·0	2·0	3·0	3·0			4·0	4·0	3·0	2·0	1·0	3·0	4·0	4·
	6·0	10·5	5·0	3·5	4·5	4·0	9·5	5·5	6·0	7·0	5·0	15·0	S.W.	11·0	8·0	8·0	3·0	4·0	4·0	7·0	10·0	8·0	4·0	6·0	6·0			8·0	7·0	8·0	3·0	6·0	3·0	7·0	10·
	7·0	5·5	5·0	3·5	2·5	2·5	10·5	4·5	5·5	6·0	3·0	8·0	W.	10·0	8·0	10·0	4·0	2·0	5·0	8·0	9·0	8·0	4·0	6·0	4·0			5·0	4·0	9·0	4·0	7·0	3·0	7·0	10·
1857	4·0	1·0	2·0	2·0	0·5	1·5	3·5	2·0	1·5	1·5	2·5	2·0	N.W.	4·0	3·0	5·0	5·0	1·0	3·0	2·0	2·0	2·0	4·0	3·0	4·0	1859	1861	2·0	2·0	5·0	4·0	4·0	2·0	2·0	3·
	2·0	2·0	1·0	2·0	2·0	3·0	2·0	4·0	3·0	2·0	3·5	1·5	C. or V.	1·0	2·0	1·0	3·0	4·0	2·0	3·0	2·0	2·0	3·0	3·0	3·0			3·0	1·0	1·0	3·0	2·0	2·0	2·0	1·
	1·25	1·77	1·88	1·18	1·79	1·65	1·55	1·04	1·38	1·29	1·10	2·32	Force of wind in lbs., ...	2·95	2·67	3·19	2·33	0·84	1·66	1·44	1·58	1·80	1·63	1·31	1·67			1·33	2·22	2·64	1·13	1·18	0·88	1·41	1·
	253·2	250·1	250·0	243·6	219·3	213·3	210·5	224·1	210·8	208·0	231·3	215·7	Deaths from all causes (all ages),	243·4	246·9	232·0	222·7	193·5	188·6	181·3	178·7	177·7	188·0	236·0	263·4			304·1	228·1	220·2	223·5	225·4	196·4	204·4	173
	243	240	240	234	210	203	202	214	204	198	220	205	Do. from all specified causes, ...	235	238	224	213	187	184	175	171	171	181	229	259			298	223	215	219	218	191	200	17
	73	70	66	60	48	44	56	65	72	62	55	43	Do. from zymotic diseases, ...	79	69	57	50	43	41	43	44	56	62	70	69			67	54	48	40	46	40	47	3
	10·1	11·0	11·8	13·8	9·0	8·2	8·8	7·7	8·9	13·0	13·8	12·0	Do. from typhus, ...	8·6	8·3	8·8	7·5	8·8	6·7	5·8	5·8	7·2	6·2	8·5	8·5			7·8	7·7	8·4	7·4	11·4	8·2	6 0	6·
	21·6	15·0	12·0	7·3	6·5	3·7	3·7	7·0	6·6	8·2	6·9	4·4	Do. from scarlatina, ...	20·4	15·0	11·2	7·0	5·3	5·2	6·1	7·8	15·1	27·4	26·8	18·1			14·5	7·0	6·1	4·4	2·6	2·4	3·1	2·
	5·1	4·7	4·5	4·9	5·0	5·0	12·3	17·9	25·9	14·0	6·0	5·0	Do. from diarrhœa, ...	3·1	3·8	3·3	2·5	1·1	4·0	7·2	8·3	8·7	5·4	3·3	2·6			2·9	2·7	1·8	2·5	2·7	3·3	6·5	5·
	40	40	46	51	45	46	47	43	37	35	40	40	Do. from tubercular diseases, ...	38	42	44	47	43	43	42	38	31	30	36	42			45	40	47	53	47	41	45	3
	27·0	26·2	33·4	37·2	31·4	33·6	32·4	27·9	23·8	24·0	27·0	28·4	Do. from phthisis pulmonalis,.....	26·8	30·2	31·6	33·8	31·9	31·0	29·6	25·3	22·1	22·0	28·0	31·7			34·8	26·7	35·3	36·6	34·6	29·8	30·7	26·
	36	37	31	29	27	25	16	14	13	18	34	35	Do. from diseases of resp. organs,	32	38	27	26	22	18	13	13	14	17	33	51			73	45	37	37	36	28	22	1
	16·1	19·4	16·6	13·4	11·7	12·0	5·8	5·6	5·4	8·6	18·2	16·4	Do. from bronchitis,.....	17·6	19·8	14·8	13·3	10·0	6·8	6·0	5·9	6·8	6·0	6·8	28·8			46·8	29·4	23·1	22·7	22·1	13·2	11·5	10·
	10·6	9·0	8·7	10·7	11·1	8·7	6·6	5·3	5·4	5·6	8·8	11·3	Do. from pneumonia, ...	9·9	11·4	7·1	8·4	7·4	7·6	5·9	5·6	3·5	5·8	9·4	12·1			13·5	10·2	9·3	9·7	9·6	7·0	7·2	3·
	124·6	122·0	116·3	108·6	100·2	90·7	100·3	114·5	111·0	93·6	101·4	94·0	Do. at 0–5 years of age,.....	125·8	120·2	108·4	97·0	83·1	76·9	84·6	82·0	80·0	87·4	107·8	114·4			125·0	111·2	102·4	96·9	102·2	90·5	95·3	72·
	24·0	22·5	25·8	24·1	22·6	25·1	20·9	19·4	19·1	22·9	20·1	19·4	Do. at 5–20 do.	22·7	25·4	21·6	23·5	23·4	21·8	18·9	20·5	21·2	25·4	23·6	23·2			25·9	21·5	24·0	26·3	25·1	21·4	22·9	19·
	63·3	63·2	67·7	70·6	61·0	63·4	56·6	59·3	51·3	57·4	61·9	59·4	Do. at 20–60 do.	60·7	61·2	66·3	62·0	56·8	56·0	48·9	49·1	48·5	48·6	63·4	77·3			88·6	59·8	60·2	62·8	65·3	51·9	53·2	54·
	41·6	41·5	39·8	41·4	35·3	34·1	32·8	31·0	30·0	34·0	47·7	44·0	Do. at 60, &c. do.	35·6	40·3	36·0	38·0	30·5	34·3	29·1	27·4	28·5	26·7	41·5	49·0			63·6	35·2	33·4	37·9	32·7	32·7	32·8	27·
	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.			Jan.	Feb.	March.	April.	May.	June.	July.	Au
	39·3	35·8	39·5	43·8	49·5	58·9	56·0	57·9	54·5	44·9	39·4	39·9	Mean temperature, ...	35·5	34·0	38·4	41·5	50·2	53·0	57·3	54·4	50·2	46·0	39·1	34·1			38·4	40·1	37·8	44·6	51·1	52·4	53·8	56·
	44·0	41·0	45·5	51·4	56·9	67·1	63·4	65·9	61·8	51·0	44·4	44·0	Mean of the highest temperatures,	39·6	39·2	44·0	48·9	57·7	59·7	64·5	60·8	57·1	51·4	43·1	37·9			42·0	44·4	42·5	51·5	58·1	58·7	60·4	62·
	34·6	30·5	33·6	36·1	42·1	50·7	48·7	49·9	47·2	39·2	34·4	35·7	Mean of the lowest temperatures,	31·4	28·6	32·5	34·0	42·6	46·5	50·1	48·0	43·3	40·5	35·1	30·0			34·8	35·8	33·2	37·8	44·0	46·0	47·3	49·
	49·0	49·0	56·0	61·0	54·7	47·0	44·0	52·0	55·0	32·9	33·3	25·2	Monthly range of temperature, ...	28·5	35·0	27·0	32·2	35·5	27·5	30·9	27·1	35·0	29·6	26·5	40·8			21·7	31·2	34·8	39·7	31·2	27·3	26·2	26·
	9·4	10·7	11·9	15·3	14·8	16·4	14·7	16·0	14·6	12·4	9·7	8·3	Mean daily range of temperature,	8·1	10·7	11·5	14·9	15·0	13·3	14·5	12·8	13·8	10·9	8·1	8·1			7·1	8·6	9·3	13·7	14·1	12·6	13·1	12·
	30·065	29·998	29·852	29·942	29·823	30·032	29·887	29 943	29·898	29·892	29·956	29·703	Mean ht. of barom.: sea-level & 32°,	29·529	29·932	29·639	29·978	29·831	29·674	29·988	29·575	29·868	29·784	29·919	29·709			29·686	30·052	29·698	29·881	29·810	29·733	29·735	29·
	1·241	1·427	1·587	1·434	1·339	0·702	0·945	1·003	0·997	1·687	1·872	1·297	Monthly range of barometer, ...	1·810	2·079	1·931	1·860	1·228	1·102	0·745	1·080	1·165	1·261	1·641	1·426			1·451	1·402	1·322	1·204	0·845	1·074	0·790	0
	14	6	13	9	16	12	17	15	14	18	10	17	Number of rainy days,.....	18	13	19	10	15	18	11	19	14	22	17	17			21	11	16	14	16	20	22	1
	2·98	1·13	1·95	1·86	2·81	2·36	4·31	2·61	2·80	4·72	2·38	4·00	Amount of rain in inches, ...	4·56	2·69	3·52	1·18	2·18	4·34	1·82	3·79	1·92	5·14	2·83	3·91			5·32	1·88	3·63	2·99	3·89	3·99	3·87	3
	86	83	85	78	81	78	83	81	84	87	89	90	Humidity (Sat. = 100), ...	89	85	86	82	81	85	83	85	87	87	90	90			90	89	88	83	84	83	83	8
	1·5	2·0	3·0	2·0	2·5	0·5	2·0	2·0	0·5	2·2	3·1	0·8	N.	3·0	5·0	3·0	4·0	1·0	2·0	3·0	2·0	3·0	2·0	2·0	4·0			2·0	2 0	3·0	3·0	1·0	3·0	1·0	2
	0·5	2·5	2·5	2·0	3·0	1·0	2·0	1·5	0·5	2·8	2·6	0·4	N.E.	3·0	2·0	2·0	4·0	2 0	3·0	2·0	1·0	2·0	1·0	3·0	3·0			1·0	2·0	6·0	2·0	2·0	1·0	1·0	2
	0·5	4·5	1·5	5·0	3·0	2·0	2·5	2·0	2·0	2·1	4·3	1·2	E.	3·0	1·0	1·0	5·0	5·0	6·0	4·0	3·0	2·0	1·0	8·0	4·0			2·0	4·0	9·0	2·0	3·0	2·0	2·0	3
	2·5	6·5	0·5	4·0	3·0	3·0	2·0	3·5	2·5	1·2	3·9	4·1	S.E.	4·0	1·0	2·0	3·0	4·0	5·0	3 0	3·0	1·0	2·0	6·0	7·0			5·0	6·0	4·0	3·0	4·0	2·0	2·0	3
	3·0	2·5	0·5	2·0	2·5	4·0	2·0	3·5	2·5	1·5	2·4	4·8	S.	4·0	2·0	3·0	2·0	4·0	4·0	3·0	2·0	2·0	4·0	2·0	3·0			5·0	3·0	2·0	4·0	5·0	3·0	3·0	3
	10·0	3·5	6·5	5·0	6·5	7·0	6·0	6·5	10·0	7·5	3·1	8·9	S.W.	5·0	4·0	6·0	3·0	5·0	4·0	3·0	5·0	6·0	8·0	2·0	2·0			6·0	3·0	3·0	7·0	6·0	6·0	7·0	7
	8·0	1·5	7·5	4·5	5·0	7·0	6·0	5·0	7·0	7·0	4·0	6·4	W.	4·0	6·0	8·0	3·0	6·0	3·0	6·0	8·0	8·0	8·0	2·0	2·0			5·0	4·0	2·0	5·0	6·0	7·0	9·0	5
1858	4·0	2·0	7·5	2·5	3·0	3·0	4·5	3·0	2·5	4·0	2·6	1·9	N.W.	3·0	6·0	5·0	3·0	2·0	2·0	4·0	4·0	4·0	3·0	2·0	3·0	1860	1862	3·0	2·0	1·0	3·0	2·0	5·0	5·0	2
	1·0	3·0	1·5	3·0	2·5	2·5	4·0	4·0	2·5	2·7	3·5	2·2	C. or V.	2·0	1·0	1·0	2·0	2·0	1·0	3·0	3·0	2·0	2·0	3·0	3·0			2·0	2·0	1·0	1·0	2·0	1·0	1·0	4
	2·36	1·80	1·89	1·45	1·34	1·36	1·11	0·99	1·68	1·90	1·09	1·80	Force of wind in lbs., ...	1·78	2·33	1·95	1·55	1·42	1·28	0·87	1·15	1·09	2·34	1·09	1·37			1·68	1·36	1·36	1·57	1·27	1·67	1·76	0
	214 0	237·7	257·8	227·1	210·4	219·3	224·0	199·5	194·6	205·3	266·1	250·0	Deaths from all causes (all ages),	280·8	330·6	283·2	290·2	240·0	219·1	214·8	192·4	182·5	210·4	230·0	257·7			296·6	251·3	256·7	250·0	228·4	215·0	192·3	16
	203	225	243	214	192	208	214	190	186	193	253	238	Do. from all specified causes, ...	273	323	276	284	236	214	210	190	179	205	222	250			290·6	248·0	252·5	245·6	223·7	211·6	189·3	16
	42	43	46	45	43	56	62	56	61	59	76	77	Do. from zymotic diseases, ...	72	66	57	57	45	48	51	46	56	70	71	71			69	60	58	50	46	39	45	
	11·9	10·0	11·5	9·0	8·1	8·7	8·2	7·0	7·3	7·2	6·9	7·9	Do. from typhus, ...	10·8	10·4	9·2	8·4	8·1	6·0	5·0	4·9	5·0	7·2	6·1	7·9			11·1	9·0	10·1	8·7	9·1	8·4	10·2	6
	3·5	3·3	3·7	3·9	2·5	3·4	3·7	4·9	11·3	17·5	29·6	26·6	Do. from scarlatina, ...	16·3	10·5	7·7	5·0	5·8	6·3	7·2	9·5	18·6	30·0	27·0	20·0			5·4	5·8	4·7	2·8	2·8	1·7	2·0	2
	4·6	4·2	5·5	3·0	3·7	7·2	11·1	9·6	11·8	5·2	4·8	2·8	Do. from diarrhœa, ...	3·4	3·6	2 5	3·7	3·4	2·4	6·4	6·1	6·1	4·8	2·3	2·4			3·3	3·4	4·0	2·8	4·0	2·8	3·8	3
	46	47	50	46	41	45	45	39	36	33	39	40	Do. from tubercular diseases, ...	45	55	52	51	53	49	45	41	34	35	38	40			45	50	48	50	50	52	39	
	32·5	34·1	35·2	34·5	28·8	32·8	30·1	23·5	22·5	22·2	28·8	29·3	Do. from phthisis pulmonalis,.....	31·3	40·4	39·1	38·1	39·5	37·1	31·3	29·0	22·4	24·4	27·2	29·2			31·1	36·6	36·4	35·1	35·5	36·9	27·9	2
	33	38	47	32	26	20	23	18	21	29	52	39	Do. from diseases of resp. organs,	59	96	63	66	41	30	27	18	15	23	34	49			68	44	48	45	34	32	26	
	18·6	18·7	23·8	15·5	11·4	9·2	9·7	7·8	10·1	12·1	29·2	21·4	Do. from bronchitis,.....	36·0	62·0	41·2	43·9	26·8	17·2	14·2	9·3	8·9	11·7	21·7	26·0			47·3	31·0	31·8	28·2	19·8	20·1	15·7	1
	10·2	12·7	14·1	10·6	9·1	7·9	9·6	6·6	8 8	12·9	14·5	11·5	Do. from pneumonia, ...	14·0	21·6	15·0	13·3	10·9	8·9	8·9	6·0	3·6	8·5	9·2	12·8			9·8	6·0	9·0	10·1	8·9	7·1	5·5	
	99·5	106·2	109·5	96·5	95·8	104·8	111·7	105·0	105·4	111·1	134·3	122·3	Do. at 0–5 years of age, ...	131·2	155·2	131·2	130·4	103·5	91·1	101·8	86·4	90·7	101·2	109·3	121·3			130·1	116·9	115·5	115·6	98·5	93·8	83·1	7
	18·7	21·2	22·8	25·1	22·3	20·7	24·3	16·8	19·7	19·7	27·8	27·9	Do. at 5–20 do.	27·4	28·3	26 9	26·8	23·3	25·0	24·7	22·1	20·9	25·6	23 9	22·6			25·0	25·7	24·7	25·0	26·0	25·6	23·6	1
	60·7	63·8	78·5	69·1	58·0	58·2	57·5	47·0	46·6	47·9	60·8	62·4	Do. at 20–60 do.	79·1	86·1	76·2	77·5	70·3	63·3	58·4	53·8	47·0	54·9	62·9	71·1			84·5	68·0	72·1	65·5	67·6	62·4	53·4	4
	34·9	45·7	46·9	36·1	33·9	35·4	30·1	30·2	22·4	26·0	43·0	36·8	Do. at 60, &c. do.	42·8	60 7	48·4	55·2	42·9	39·5	29·9	29·8	23·9	27·3	33·7	42·6			57·1	40·9	44·8	44·5	36·0	33·5	32·3	2

THE METEOROLOGY AND THE DEATH-RATE (FROM ALL AND SEVERAL CAUSES AT ALL AGES, AND FROM ALL CAUSES AT SEVERAL AGES), IN THE CONSECUTIVE ORDER OF THE MONTHS IN EACH YEAR.

v.	Dec.		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
7	44·9	Mean temperature, ...	39·6	40·5	43·0	41·3	51·9	56·65	59·0	57·8	52·3	45·8	39·4	34·0
3	49·2	Mean of the highest temperatures,	44·4	45·4	48·5	48·7	61·7	64·7	66·2	65·2	59·2	51 8	44·5	38·3
2	40·7	Mean of the lowest temperatures,	34·8	35·5	37·6	35·2	42·1	48·6	51·9	50·4	45·5	39·8	34·2	29·7
9	38·0	Monthly range of temperature, ...	27·1	29·0	33·2	39·0	42·4	35·3	32·4	30·7	28·2	43·8	26·9	35·5
	8·5	Mean daily range of temperature,	9 6	9·8	10·9	13·5	19·7	16·1	14·3	14·7	13·6	12·0	10·4	8·7
15	29·989	Mean ht. of barom.: sea-level & 32°,	29·864	29·709	29·707	29·751	30·046	29·934	30·050	29·850	29·722	29·667	29·855	29·651
719	1·338	Monthly range of barometer, ...	1·855	1·375	1·270	1·289	0·498	0·653	1·083	1·167	1·063	1·236	2·129	1·853
2	14	Number of rainy days, ...	17	17	11	15	4	11	14	14	17	14	14	15
89	3·37	Rain in inches, ...	4·21	3·38	4·18	3·20	0·29	2·04	2·76	2·27	3·21	4·60	3·37	3·66
0	88	Humidity (Sat. = 100), ...	87	88	85	80	73	78	81	80	82	89	89	88
5	0·5	No. of (Winds) Days. N.	1·0	1·0	2·0	4 0	2·0	2·0	1·0	0·0	0·0	3·0	3·0	4·0
5	0·0	N.E.	0·0	0·0	1·0	3·0	3·0	4·0	2·0	0·0	1·0	3·0	1·0	2·0
5	0·0	E.	0·0	1·0	1·0	5·0	6·0	5·0	4·0	1·0	2·0	4·0	2·0	2·0
5	1·0	S.E.	1·0	2·0	1·0	2·0	5·0	3·0	2·0	1·0	3·0	4·0	3·0	3·0
0	3·0	S.	3·0	3·0	2·0	1·0	4·0	2·0	2·0	5·0	4·0	2·0	3·0	3·0
0	15·0	S.W.	11·0	8·0	8·0	3·0	4·0	4·0	7·0	10·0	8·0	4·0	6·0	6·0
0	8·0	W.	10·0	8·0	10·0	4·0	2·0	5·0	8·0	9·0	8·0	4·0	6·0	4·0
5	2·0	N.W.	4·0	3·0	5·0	5·0	1·0	3·0	2·0	2·0	2·0	4·0	3·0	4·0
5	1·5	C. or V.	1·0	2·0	1·0	3·0	4·0	2·0	3·0	2·0	2·0	3·0	3·0	3·0
10	2·32	Force of wind in lbs., ...	2·95	2·67	3·12	2·33	0·84	1·66	1·44	1·58	1·80	1·63	1·31	1·67
·3	215·7	Deaths from all causes (all ages),	243·4	246·9	232·0	222·7	193·5	188·6	181·3	178·7	177·7	188·0	236·0	263·4
0	205	Do. from all specified causes, ...	235	238	224	213	187	184	175	171	171	181	229	259
5	43	Do. from zymotic diseases, ...	79	69	57	50	42	41	43	44	56	62	70	69
8	12·0	Do. from typhus, ...	8·6	8·3	8·8	7·5	8·8	6·7	5·8	5·8	7·2	6·2	8·5	8·5
9	4·4	Do. from scarlatina, ...	20·4	15·0	11·2	7·0	5·3	5·2	6·1	7·8	15·1	27·4	26·8	18·1
0	5·0	Do. from diarrhœa, ...	3·1	3·8	3·3	2·5	1·1	4·0	7·2	8·3	8·7	5·4	3·3	2·6
0	40	Do. from tubercular diseases, ...	38	42	44	47	43	43	42	38	31	30	36	42
0	28·4	Do. from phthisis pulmonalis, ...	26·8	30·2	31·6	33·8	31·9	31·0	29·6	25·3	22·1	22·0	28·0	31·7
4	35	Do. from diseases of resp. organs,	32	38	27	26	22	18	13	13	14	17	33	51
2	16·4	Do. from bronchitis, ...	17·6	19·8	14·8	13·3	10·0	6·8	6·0	5·9	6·8	6·0	6·8	28·8
8	11·3	Do. from pneumonia, ...	9·9	11·4	7·1	8·4	7·4	7·6	5·9	5·6	3·5	5·8	9·4	12·1
·4	94·0	Do. at 0–5 years of age, ...	125·8	120·2	108·4	97·0	83·1	76·9	84·6	82·0	80·0	87·4	107·8	114·4
1	19·4	Do. at 5–20 do.	22·7	25·4	21·6	23·5	23·4	21·8	18·9	20·5	21·2	25·4	23·6	23·2
9	59·4	Do. at 20–60 do.	60·7	61·2	66·3	62·0	56·8	56·0	48·9	49·1	48·5	48·6	63·4	77·3
7	44·0	Do. at 60, &c. do.	35·6	40·3	36·0	38·0	30·5	34·3	29·1	27·4	28·5	26·7	41·5	49·0

1859 — Ratio of Deaths per 100,000 living at all Ages.

v.	Dec.		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
4	39·9	Mean temperature, ...	35·5	34·0	38·4	41·5	50·2	53·0	57·3	54·4	50·2	46·0	39·1	34·1
4	44·0	Mean of the highest temperatures,	39·6	39·2	44·0	48·9	57·7	59·7	64·5	60·8	57·1	51·4	43·1	37·9
4	35·7	Mean of the lowest temperatures,	31·4	28·6	32·5	34·0	42·6	46·5	50·1	48·0	43·3	40·5	35·1	30·0
3	25·2	Monthly range of temperature, ...	28·5	35·0	27·0	32·2	35·5	27·5	30·9	27·1	35·0	29·6	26·5	40·8
7	8·3	Mean daily range of temperature,	8·1	10·7	11·5	14·9	15·0	13·3	14·5	12·8	13·8	10·9	8·1	8·1
·956	29·703	Mean ht. of barom.: sea-level & 32°,	29·529	29·932	29·639	29·978	29·831	29·674	29·988	29·575	29·868	29·784	29·919	29·709
·872	1·297	Monthly range of barometer, ...	1·810	2·079	1·931	1·860	1·228	1·102	0·745	1·080	1·165	1·261	1·641	1·426
0	17	Number of rainy days, ...	18	13	19	10	15	18	11	19	14	22	17	17
·38	4·00	Amount of rain in inches, ...	4·56	2·69	3·52	1·18	2·18	4·34	1·82	3·79	1·92	5·14	2·83	3·91
89	90	Humidity (Sat. = 100), ...	89	85	86	82	81	85	83	85	87	87	90	90
·1	0·8	No. of (Winds) Days. N.	3·0	5·0	3·0	4·0	1·0	2·0	3·0	2·0	3·0	2·0	2·0	4·0
·6	0·4	N.E.	3·0	2·0	2·0	4·0	2 0	3·0	2·0	1·0	2·0	1·0	3·0	3·0
·3	1·2	E.	3·0	1·0	1·0	5·0	5·0	6·0	4·0	3·0	2·0	1·0	8·0	4·0
·9	4·1	S.E.	4·0	1·0	2·0	3·0	4·0	5·0	3 0	3·0	1·0	2·0	6·0	7·0
·4	4·8	S.	4·0	2·0	3·0	2·0	4·0	4·0	3·0	2·0	2·0	4·0	2·0	3·0
·1	8·9	S.W.	5·0	4·0	6·0	3·0	5·0	4·0	3·0	5·0	6·0	8·0	2·0	2·0
·0	6·4	W.	4·0	6·0	8·0	3·0	6·0	3·0	6·0	8·0	8·0	8·0	2·0	2·0
·6	1·9	N.W.	3·0	6·0	5·0	3·0	2·0	2·0	4·0	4·0	4·0	3·0	2·0	3·0
·5	2·2	C. or V.	2·0	1·0	1·0	2·0	2·0	1·0	3·0	3·0	2·0	2·0	3·0	3·0
·09	1·80	Force of wind in lbs., ...	1·78	2·33	1·95	1·55	1·42	1·28	0·87	1·15	1·09	2·34	1·09	1·37
6·1	250·0	Deaths from all causes (all ages),	280·8	330·6	283·2	290·2	240·0	219·1	214·8	192·4	182·5	210·4	230·0	257·7
53	238	Do. from all specified causes, ...	273	323	276	284	236	214	210	190	179	205	222	250
76	77	Do. from zymotic diseases, ...	72	66	57	57	45	48	51	46	56	70	71	71
·9	7·9	Do. from typhus, ...	10·8	10·4	9·2	8·4	8·1	6·0	5·0	4·9	5·0	7·2	6·1	7·9
·6	26·6	Do. from scarlatina, ...	16·3	10·5	7·7	5·0	5·8	6·3	7·2	9·5	18·6	30·0	27·0	20·0
·8	2·8	Do. from diarrhœa, ...	3·4	3·6	2 5	3·7	3·4	2·4	6·4	6·1	6·1	4·8	2·3	2·4
39	40	Do. from tubercular diseases, ...	45	55	52	51	53	49	45	41	34	35	38	40
·8	29·3	Do. from phthisis pulmonalis, ...	31·3	40·4	39·1	38·1	39·5	37·1	31·3	29·0	22·4	24·4	27·2	29·2
52	39	Do. from diseases of resp. organs,	59	96	63	66	41	30	27	18	15	23	34	49
·2	21·4	Do. from bronchitis, ...	36·0	62·0	41·2	43·9	26·8	17·2	14·2	9·3	8·9	11·7	21·7	26·0
·5	11·5	Do. from pneumonia, ...	14·0	21·6	15·0	13·3	10·9	8·9	8·9	6·0	3·6	8·5	9·2	12·8
4·3	122·3	Do. at 0–5 years of age, ...	131·2	155·2	131·2	130·4	103·5	91·1	101·8	86·4	90·7	101·2	109·3	121·3
·8	27·9	Do. at 5–20 do.	27·4	28·3	26·9	26·8	23·3	25·0	24·7	22·1	20·9	25·6	23·9	22·6
·8	62·4	Do. at 20–60 do.	79·1	86·1	76·2	77·5	70·3	63·3	58·4	53·8	47·0	54·9	62·9	71·1
0	36·8	Do. at 60, &c. do.	42·8	60·7	48·4	55·2	42·9	39·5	29·9	29·8	23·9	27·3	33·7	42·6

1860 — Ratio of Deaths per 100,000 living at all Ages.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Mean temperature.	36·3	39·5	41·1	45·4	49·1	57·2	56·8	57·4	53·7	49·5	38·5	38·0
Mean of the highest temperatures,	40·4	44·1	46·9	52·4	56·9	64·0	63·6	62·8	59·2	55·4	43·9	42·8
Mean of the lowest temperatures,	32·9	34·9	35·5	38·3	41·3	50·4	50·0	51·9	48·2	43·7	33·0	33·3
Monthly range of temperature, ...	37·4	29·7	24·2	34·0	39·0	32·3	27·6	25·1	27·7	30·9	33·4	30·6
Mean daily range of temperature,	8·2	9·3	11·4	14·1	15·6	13·6	13·6	11·0	11·0	11·6	10·9	9·5
Mean ht. of barom.: sea-level & 32°,	30·038	29·681	29·507	30·177	30·070	29·961	29·619	29·774	29·723	29·936	29·544	30·020
Monthly range of barometer, ...	1·026	1·670	1·430	0·734	0·937	0·694	0·777	0·797	1·172	1·201	1·295	1·459
Number of rainy days (total), ...	14	15	22	9	14	13	20	22	19	14	19	13
Amount of rain in inches (total), ...	3·09	3·32	5·10	1·04	1·57	2·35	3·94	6·44	5·27	3·34	6·63	2·98
Humidity (Sat. = 100), ...	90	88	86	83	80	84	84	86	88	90	87	90
No. of (Winds) Days. N.	2·0	2·0	1·0	3·0	4·0	3·0	1·0	1·0	3·0	1·0	4·0	3·0
N.E.	1·0	2·0	1·0	3·0	3·0	4·0	2·0	0·0	1·0	1·0	3·0	2·0
E.	3·0	2·0	1·0	5·0	3·0	7·0	3·0	1·0	2·0	2·0	2·0	1·0
S.E.	3·0	4·0	2·0	3·0	1·0	4·0	3·0	1·0	3·0	4·0	1·0	2·0
S.	4·0	4·0	3·0	2·0	1·0	3·0	4·0	4·0	4·0	6·0	2·0	3·0
S.W.	8·0	7·0	8·0	3·0	6·0	3·0	7·0	10·0	6·0	8·0	7·0	7·0
W.	5·0	4·0	9·0	4·0	7·0	3·0	7·0	10·0	6·0	5·0	6·0	6·0
N.W.	2·0	2·0	5·0	4·0	4·0	2·0	2·0	3·0	3·0	1·0	3·0	3 0
C. or V.	3·0	1·0	1·0	3·0	2·0	2·0	2·0	1·0	2·0	3·0	2·0	4·0
Force of wind in lbs., ...	1·33	2·22	2·64	1·13	1·18	0·88	1·41	1·92	1·38	1·28	2·36	1·20
Deaths from all causes (all ages),	304·1	228·1	220·2	223·5	225·4	196·4	204·4	173·1	165·0	173·1	224·5	241·4
Do. from all specified causes, ...	298	223	215	219	218	191	200	170	162	170	220	237
Do. from zymotic diseases, ...	67	54	48	40	46	40	47	38	36	42	54	52
Do. from typhus, ...	7·8	7·7	8·4	7·4	11·4	8·2	6·0	6·6	5·7	6·9	10·9	9·2
Do. from scarlatina, ...	14·5	7·0	6·1	4·4	2·6	2·4	3·6	2·8	3·4	6·2	7·7	5·8
Do. from diarrhœa, ...	2·9	2·7	1·8	2·5	2·7	3·3	6·5	5·6	5·2	5·0	2·9	2·9
Do. from tubercular diseases, ...	45	40	47	53	47	41	45	39	31	31	36	35
Do. from phthisis pulmonalis, ...	34·8	26·7	35·3	36·6	34·6	29·8	30·7	26·8	21·4	22·0	25·3	25·4
Do. from diseases of resp. organs,	73	45	37	37	36	28	22	17	18	24	42	56
Do. from bronchitis, ...	46·8	29·4	23·1	22·7	22·1	13·2	11·5	10·4	11·2	16·1	28·8	35·1
Do. from pneumonia, ...	13·5	10·2	9·3	9·7	9·6	7·0	7·2	3·6	5·5	5·2	8·5	11·4
Do. at 0–5 years of age, ...	125·0	111·2	102·4	96·9	102·2	90·5	95·3	72·5	74·2	78·7	101·7	107·5
Do. at 5–20 do.	25·9	21·5	24·0	26·3	25·1	21·4	22·9	19·3	16·8	17·8	17·7	19·7
Do. at 20–60 do.	88·6	59·8	60·2	62·8	65·3	51·9	53·2	54·3	45·0	48·7	60·2	69·8
Do. at 60, &c. do.	63·6	35·2	33·4	37·9	32·7	32·7	32·8	27·0	28·6	27·5	44·6	44·1

1861 — Ratio of Deaths per 100,000 living at all Ages.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Mean temperature, ...	38·4	40·1	37·8	44·6	51·1	52·4	53·8	56·0	53·4	47·1	37·1	41·8
Mean of the highest temperatures,	42·0	44·4	42·5	51·5	58·1	58·7	60·4	62·3	60·3	53·4	42·6	46·2
Mean of the lowest temperatures,	34·8	35·8	33·2	37·8	44·0	46·0	47·3	49·7	46·4	40·7	31·6	37·4
Monthly range of temperature, ...	21·7	31·2	34·8	39·7	31·2	27·3	26·2	26·4	32·9	32·8	33·3	22·7
Mean daily range of temperature,	7·1	8·6	9·3	13·7	14·1	12·6	13·1	12·6	14·1	12·7	11·0	8·7
Mean ht. of barom.: sea-level & 32°,	29·686	30·052	29·698	29·881	29·810	29·733	29·735	29·885	29·979	29·620	29·897	29·767
Monthly range of barometer, ...	1·451	1·402	1·322	1·204	0·845	1·074	0·790	0·944	0·993	1·872	1·513	1·204
Number of rainy days (total), ...	21	11	16	14	16	20	22	16	13	10	19	21
Amount of rain in inches (total), ...	5·32	1·88	3·63	2·99	3·89	3·99	3·87	3·35	2·44	2·41	6·32	5·20
Humidity (Sat. = 100), ...	90	89	88	83	84	83	83	87	88	88	90	89
No. of (Winds) Days. N.	2·0	2 0	3·0	3·0	1·0	3·0	1·0	2·0	1·0	2·0	1·0	1·0
N.E.	1·0	2·0	6·0	2·0	2·0	1·0	1·0	2·0	2·0	1·0	0·0	0·0
E.	2·0	4·0	9·0	2·0	3·0	2·0	2·0	3·0	3·0	2·0	1·0	1·0
S.E.	5·0	6·0	4·0	3·0	4·0	2·0	2·0	3·0	4·0	4·0	3·0	3·0
S.	5·0	3·0	2·0	4·0	5·0	3·0	3·0	3·0	3·0	4·0	3·0	3·0
S.W.	6·0	3·0	3·0	7·0	6·0	6·0	7·0	7·0	6·0	6·0	9·0	9·0
W.	5·0	4·0	2·0	5·0	6·0	7·0	9·0	5·0	5·0	4·0	9·0	9·0
N.W.	3·0	2·0	1·0	3·0	2·0	5·0	5·0	2·0	2·0	3·0	3·0	4·0
C. or V.	2·0	2·0	1·0	1·0	2·0	1·0	1·0	4·0	4·0	4·0	2·0	1·0
Force of wind in lbs., ...	1·68	1·36	1·36	1·57	1·27	1·67	1·76	0·82	0·92	0·80	2·07	2·15
Deaths from all causes (all ages),	296·6	251·3	256·7	250·0	228·4	215·0	192·3	168·4	195·9	204·4	234·7	259·0
Do. from all specified causes, ...	290·6	248·0	252·5	245·6	223·7	211·6	189·3	165·5	193·2	200·5	231·7	255·2
Do. from zymotic diseases, ...	69	60	58	50	46	39	45	36	47	59	69	76
Do. from typhus, ...	11·1	9·0	10·1	8·7	9·1	8·4	10·2	6·8	7·2	8·7	8·5	9·5
Do. from scarlatina, ...	5·4	5·8	4·7	2·8	2·8	1·7	2·0	2·6	3·0	4·0	7·0	5·8
Do. from diarrhœa, ...	3·3	3·4	4·0	2·8	4·0	2·8	3·8	3·9	6·0	5·8	3·4	4·1
Do. from tubercular diseases, ...	45	50	48	50	50	52	39	35	39	34	33	37
Do. from phthisis pulmonalis, ...	31·1	36·6	36·4	35·1	35·5	36·9	27·9	23·4	26·5	23·2	24·3	24·7
Do. from diseases of resp. organs,	68	44	48	45	34	32	26	21	22	29	46	52
Do. from bronchitis, ...	47·3	31·0	31·8	28·2	19·8	20·1	15·7	13·5	15·0	19·8	31·0	34·0
Do. from pneumonia, ...	9·8	6·0	9·0	10·1	8·9	7·1	5·5	5·2	5·0	5·1	7·9	10·5
Do. at 0–5 years of age, ...	130·1	116·9	115·5	115·6	98·5	93·8	83·1	74·5	91·7	105·2	114·3	124·0
Do. at 5–20 do.	25·0	25·7	24·7	25·0	26·0	25·6	23·6	19·9	24·4	17·7	21·1	23·5
Do. at 20–60 do.	84·5	68·0	72·1	65·5	67·6	62·4	53·4	47·0	53·0	48·8	62·4	67·2
Do. at 60, &c. do.	57·1	40·9	44·8	44·5	36·0	33·5	32·3	27·1	26·9	31·3	37·1	44·7

1862 — Ratio of Deaths per 100,000 living at all Ages.

	Means of the several Years. 1857.	1859.	1861.	Mean of all the Years.
Mean temperature.	48·0	46·7	46·9	46·5
Mean of the highest temperatures,	53·9	53·2	52·7	52·5
Mean of the lowest temperatures,	42·1	40·4	41·1	40·4
Monthly range of temperature, ...	49·8	33·6	30·9	36·9
Mean daily range of temperature,	11·8	12·7	11·6	12·0
Mean ht. of barom.: sea-level & 32°,	29·893	29·817	29·838	29·843
Monthly range of barometer, ...	1·236	1·289	1·099	1·263
Number of rainy days (total), ...	163	163	194	179
Amount of rain in inches (total), ...	30·56	37·17	45·07	38·31
Humidity (Sat. = 100), ...	85·6	83·3	86·4	85·3
No. of (Winds) Days. N.	20·3	23·0	28·0	24·9
N.E.	31·5	20·0	23·0	23·9
E.	47·0	33·0	32·0	36·6
S.E.	34·5	30·0	31·0	36·0
S.	35·5	34·0	40·0	36·1
S.W.	81·5	79·0	80·0	74·8
W.	63·5	78·0	72·0	69·4
N.W.	24·0	38·0	34·0	35·4
C. or V.	28·0	29·0	26·0	27·6
Force of wind in lbs., ...	1·52	1·92	1·57	1·59
Deaths from all causes (all ages),	227·5	212·7	214·9	225·7
Do. from all specified causes, ...	217·8	205·6	210·2	218·5
Do. from zymotic diseases, ...	59·5	57·0	47·0	55·4
Do. from typhus, ...	10·7	7·5	8·0	8·5
Do. from scarlatina, ...	8·6	13·7	5·5	9·2
Do. from diarrhœa, ...	9·2	4·4	3·7	5·2
Do. from tubercular diseases, ...	42·5	39·7	40·8	42·1
Do. from phthisis pulmonalis, ...	29·4	28·7	29·1	29·7
Do. from diseases of resp. organs,	26·2	25·3	36·2	33·6
Do. from bronchitis, ...	12·4	11·9	22·9	19 1
Do. from pneumonia, ...	8·5	7·8	8·4	8·9
Do. at 0–5 years of age, ...	106·4	97·3	96·6	104·5
Do. at 5–20 do.	22·1	22·6	21·5	22·7
Do. at 20–60 do.	61·2	58·2	60·0	61·3
Do. at 60, &c. do.	37·8	34·7	36 7	36·9

	1858.	1860.	1862.
Mean temperature, ...	46·6	44·5	46·1
Mean of the highest temperatures,	53·0	50·3	51 8
Mean of the lowest temperatures,	40·2	38·5	40·4
Monthly range of temperature, ...	45·8	31·3	30·0
Mean daily range of temperature,	12·8	11·8	11·5
Mean ht. of barom.: sea-level & 32°,	29·916	29·785	29·812
Monthly range of barometer, ...	1·294	1·444	1·218
Number of rainy days (total), ...	161	193	199
Amount of rain in inches (total), ...	108·5	112·8	105·3
Humidity (Sat. = 100), ...	83·8	86·0	86·8
No. of (Winds) Days. N.	22·1	34·0	22·0
N.E.	21·3	28·0	20·0
E.	30·6	43·0	34·0
S.E.	36·7	41·0	43·0
S.	31·2	35·0	41·0
S.W.	80·5	53·0	75·0
W.	68·9	64·0	70 0
N.W.	40·5	41·0	35·0
C. or V.	32·4	25·0	25·0
Force of wind in lbs., ...	1·56	1·52	1·45
Deaths from all causes (all ages),	225·5	244·2	229·2
Do. from all specified causes, ...	213·3	238·5	225·6
Do. from zymotic diseases, ...	55·5	59·1	54·5
Do. from typhus, ...	8·6	7·4	9·0
Do. from scarlatina, ...	9·5	13·8	4·0
Do. from diarrhœa, ...	6·1	3·9	4·0
Do. from tubercular diseases, ...	42·2	44·8	42·7
Do. from phthisis pulmonalis, ...	29·5	32·4	30·1
Do. from diseases of resp. organs,	31·5	43·4	39·0
Do. from bronchitis, ...	15·6	26·6	25·6
Do. from pneumonia, ...	10·7	11·0	7·5
Do. at 0–5 years of age, ...	108·5	112·8	105·3
Do. at 5–20 do.	22·2	24·8	23·5
Do. at 20–60 do.	59·2	66·7	62·7
Do. at 60, &c. do.	35·1	39·6	38·0

TABLE C. In which the corresponding Months of the several Years are arranged in Groups.

Month.	Year.	Mean Temperature.	Mean of the highest Temperatures.	Mean of the lowest Temperatures.	Monthly Range of Temperature.	Mean of the Daily Range of Temperature.	Mean Height of Barometer at Sea level, and 32°.	Monthly Range of Barometer.	Number of Rainy Days.	Rain in inches.	Humidity, Sat.=100.	WINDS. N.	NE.	E.	SE.	S.	SW.	W.	NW.	Calm or Variable.	Force of Wind in lbs.	DEATHS IN 100,000 LIVING AT ALL AGES. AT ALL AGES FROM ALL AND DIFFERENT CAUSES. From all Causes.	From all Specified Causes.	From Zymotic Diseases.	From Typhus.	From Scarlatina.	From Diarrhœa.	From Tubercular Diseases.	From Phthisis Pulmonalis.	From Diseases of Respiratory Organs.	From Bronchitis.	From Pneumonia.	ALL CAUSES AT DIFFERENT AGES. 0-5.	5-20.	20-60.	60, &c.
January	1857	35·7	39·8	31·6	50·1	8·2	29·698	1·534	16	2·77	87	3·5	3·5	2·0	1·5	2·0	6·0	7·0	4·0	2·0	1·25	253·2	243	73	10·1	21·6	5·1	40	27·0	36	16·1	10·6	124·6	24·0	63·3	41·6
	1858	39·3	44·0	34·6	40·0	9·4	30·065	1·241	14	2·98	86	1·5	0·5	0·5	2·5	3·0	10·0	8·0	4·0	1·0	2·36	214·0	203	42	11·9	3·5	4·6	46	32·5	33	18·6	10·2	99·5	18·7	60·7	34·9
	1859	39·6	44·4	34·8	27·1	9·6	29·864	1·855	17	4·21	87	1·0	0·0	0·0	1·0	3·0	11·0	10·0	4·0	1·0	2·95	243·4	235	79	8·6	20·4	3·1	38	26·8	32	17·6	9·9	125·8	22·7	60·7	35·6
	1860	35·5	39·6	31·4	28·5	8·1	29·529	1·810	18	4·56	89	3·0	3·0	3·0	4·0	4·0	5·0	4·0	3·0	2·0	1·78	289·8	273	72	10·8	16·3	3·4	45	31·3	59	36·0	14·0	131·2	27·4	79·1	42·8
	1861	36·3	40·4	32·9	37·4	8·2	30·038	1·026	14	3·09	90	2·0	1·0	3·0	3·0	4·0	8·0	5·0	2·0	3·0	1·33	304·1	298	67	7·8	14·5	2·9	45	34·8	73	46·8	13·5	125·0	25·9	88·6	63·6
	1862	38·4	42·0	34·8	21·7	7·1	29·686	1·451	21	5·32	90	2·0	1·0	2·0	5·0	5·0	6·0	5·0	3·0	2·0	1·68	296·6	291	69	11·1	5·4	3·3	45	31·1	68	47·3	9·8	130·1	25·0	84·5	57·1
	Means	37·4	41·7	33·4	34·1	8·4	29·813	1·486	16·6	3·82	88·1	1·5	1·5	1·7	2·8	3·5	7·6	6·5	3·3	1·8	1·89	265·3	257·1	67·0	10·0	13·6	3·7	43·2	30·6	50·2	30·4	11·3	122·7	23·9	72·8	45·9
February	1857	39·3	44·3	34·3	56·0	10·8	29·840	1·289	11	1·54	89	0·5	0·0	1·0	2·5	5·0	10·5	5·5	1·0	2·0	1·77	250·1	240	70	11·0	15·0	4·7	40	26·2	37	19·4	9·0	122·0	22·5	63·2	41·5
	1858	35·8	41·0	30·5	49·0	10·7	29·998	1·427	6	1·13	83	2·0	2·5	4·5	6·5	2·5	3·5	1·5	2·0	3·0	1·80	237·7	225	43	10·0	3·3	4·2	47	34·1	38	18·7	12·7	106·2	21·2	63·8	45·7
	1859	40·5	45·4	35·5	29·0	9·8	29·709	1·375	17	3·38	88	1·0	0·0	1·0	2·0	3·0	8·0	8·0	3·0	2·0	2·67	246·9	238	69	8·3	15·0	3·8	42	30·2	33	19·8	11·4	120·2	25·4	61·2	40·3
	1860	34·0	39·2	28·6	35·0	10·7	29·932	2·079	13	2·69	85	5·0	2·0	1·0	1·0	2·0	4·0	6·0	6·0	1·0	2·33	330·6	323	66	10·4	10·5	3·6	55	40·4	96	62·0	21·6	155·2	28·3	86·1	60·7
	1861	39·5	44·1	34·9	29·7	9·3	29·681	1·670	15	3·32	88	2·0	2·0	2·0	4·0	4·0	7·0	4·0	2·0	1·0	2·22	228·1	223	54	7·7	7·0	2·7	40	26·7	45	29·4	10·2	111·2	21·5	59·8	35·2
	1862	40·1	44·4	35·8	31·2	8·6	30·052	1·402	11	1·88	89	2·0	2·0	4·0	6·0	3·0	3·0	4·0	2·0	2·0	1·36	251·3	248	60	9·0	5·8	3·4	50	36·6	44	31·0	6·0	116·9	25·7	68·0	40·9
	Means	38·2	43·1	33·2	38·3	10·0	29·869	1·540	12·1	2·32	87·0	2·1	1·6	2·2	3·7	3·2	6·0	4·8	2·6	1·8	2·02	257·4	249·5	60·3	9·4	9·4	3·7	45·7	32·3	49·7	30·0	11·8	121·9	24·1	67·0	44·0
March	1857	39·2	44·0	34·5	49·5	9·5	29·804	2·021	16	2·94	86	1·0	3·0	7·0	4·0	3·0	5·0	5·0	2·0	1·0	1·88	250·0	240	66	11·8	12·0	4·5	46	33·4	31	16·6	8·7	116·3	25·8	67·7	39·8
	1858	39·5	45·5	33·6	56·0	11·9	29·852	1·587	13	1·95	85	3·0	2·5	1·5	0·5	0·5	6·5	7·5	7·5	1·5	1·89	257·8	243	46	11·5	3·7	5·5	50	35·2	47	23·8	14·1	109·5	22·8	78·5	46·9
	1859	43·0	48·5	37·6	33·2	10·9	29·707	1·270	11	4·18	85	2·0	1·0	1·0	1·0	2·0	8·0	10·0	5·0	1·0	3·19	232·0	224	57	8·8	11·2	3·3	44	31·6	27	14·8	7·1	108·4	21·6	66·3	36·0
	1860	38·4	44·0	32·5	27·0	11·5	29·639	1·931	19	3·52	86	3·0	2·0	1·0	2·0	3·0	6·0	8·0	5·0	1·0	1·95	283·2	276	57	9·2	7·7	2·5	52	39·1	63	41·2	15·0	131·2	26·9	76·2	48·4
	1861	41·1	46·9	35·5	24·2	11·4	29·507	1·430	22	5·10	86	1·0	1·0	1·0	2·0	3·0	8·0	9·0	5·0	1·0	2·64	220·2	215	48	8·4	6·1	1·8	47	35·3	37	23·1	9·3	102·4	24·0	60·2	33·4
	1862	37·8	42·5	33·2	34·8	9·3	29·698	1·322	16	3·63	88	3·0	6·0	9·0	4·0	2·0	3·0	2·0	1·0	1·0	1·36	256·7	252	58	10·1	4·7	4·0	48	36·4	48	31·8	9·0	115·5	24·7	72·1	44·8
	Means	39·8	45·2	34·5	37·3	10·7	29·701	1·593	16·1	3·55	86·0	2·2	2·6	3·6	2·2	2·2	6·1	6·9	4·2	1·1	2·15	249·8	241·6	55·3	9·9	7·6	3·6	47·8	35·1	42·2	25·2	10·5	113·9	24·3	70·2	41·5
April	1857	42·7	48·7	36·7	46·0	12·0	29·767	1·142	16	2·38	84	2·0	3·0	7·0	4·0	3·0	3·5	3·5	2·0	2·0	1·18	243·6	234	60	13·8	7·3	4·9	51	37·2	29	13·4	10·7	108·6	24·1	70·6	41·4
	1858	43·8	51·4	36·1	61·0	15·3	29·942	1·434	9	1·86	78	2·0	2·0	5·0	4·0	2·0	5·0	4·5	2·5	3·0	1·45	227·1	214	45	9·0	3·9	3·0	46	34·5	32	15·5	10·6	96·5	25·1	69·1	36·1
	1859	41·3	48·7	35·2	39·0	13·5	29·751	1·289	15	3·20	80	4·0	3·0	5·0	2·0	1·0	3·0	4·0	5·0	3·0	2·33	222·7	213	50	7·5	7·0	2·5	47	33·8	26	13·3	8·4	97·0	23·5	62·0	38·0
	1860	41·5	48·9	34·0	32·2	14·9	29·978	1·860	10	1·18	82	4·0	4·0	5·0	3·0	2·0	3·0	3·0	3·0	2·0	1·55	290·2	284	57	8·4	5·0	3·7	51	38·1	66	43·9	13·3	130·4	26·8	77·5	55·2
	1861	45·4	52·4	38·3	34·0	14·1	30·177	0·734	9	1·04	83	3·0	3·0	5·0	3·0	2·0	3·0	4·0	4·0	3·0	1·13	223·5	219	40	7·4	4·4	2·5	53	36·6	37	22·7	9·7	96·9	26·3	62·8	37·9
	1862	44·6	51·5	37·8	39·7	13·7	29·881	1·204	14	2·99	83	3·0	2·0	2·0	3·0	4·0	7·0	5·0	3·0	1·0	1·57	250·0	246	50	8·7	2·8	2·8	50	35·1	45	28·2	10·1	115·6	25·0	65·5	44·5
	Means	43·2	50·3	36·3	41·9	13·9	29·916	1·277	12·2	2·11	81·7	3·0	2·8	4·8	3·1	2·3	4·1	4·0	3·2	2·3	1·53	242·8	235·0	50·3	9·1	5·1	3·2	49·7	35·9	39·2	22·8	10·4	107·5	25·1	67·9	42·1
May	1857	49·8	57·0	42·6	47·0	14·4	29·959	0·890	13	1·66	80	1·5	4·0	8·5	4·5	3·0	4·5	2·5	0·5	2·0	1·79	219·3	210	48	9·0	6·5	5·0	45	31·4	27	11·7	11·1	100·2	22·6	61·0	35·3
	1858	49·5	56·9	42·1	54·7	14·8	29·823	1·339	16	2·81	81	2·5	3·0	3·0	3·0	2·5	6·5	5·0	3·0	2·5	1·34	210·4	192	43	8·1	2·5	3·7	41	28·8	26	11·4	9·1	95·8	22·3	58·0	33·9
	1859	51·9	61·7	42·1	42·4	19·7	30·046	0·498	4	0·29	73	2·0	3·0	6·0	5·0	4·0	4·0	2·0	1·0	4·0	0·84	193·5	187	45	8·8	5·3	1·1	43	31·9	22	10·0	7·4	83·1	23·4	56·8	30·5
	1860	50·2	57·7	42·6	35·5	15·0	29·831	1·228	15	2·18	81	1·0	2·0	5·0	4·0	4·0	5·0	6·0	2·0	2·0	1·42	240·0	236	45	8·1	5·8	3·4	53	39·5	41	26·8	10·9	103·5	23·3	70·3	42·9
	1861	49·1	56·9	41·3	39·0	15·6	30·070	0·937	14	1·57	80	4·0	3·0	3·0	1·0	1·0	6·0	7·0	4·0	2·0	1·18	225·4	218	46	11·4	2·6	2·7	47	34·6	36	22·1	9·6	102·2	25·1	65·3	32·7
	1862	51·1	58·1	44·0	31·2	14·1	29·810	0·845	16	3·89	84	1·0	2·0	3·0	4·0	5·0	6·0	6·0	2·0	2·0	1·27	228·4	224	46	9·1	2·8	4·0	50	35·5	34	19·8	8·9	98·5	26·0	67·6	36·0
	Means	50·3	58·0	42·4	41·6	15·6	29·923	0·956	13·0	2·07	79·8	2·0	2·8	4·7	3·6	3·2	5·3	4·7	2·1	2·4	1·31	219·5	211·2	45·0	9·1	4·2	3·3	46·5	33·6	31·0	16·9	9·5	97·2	23·8	63·2	35·2
June	1857	57·4	66·0	48·8	66·5	17·2	30·020	1·039	9	2·79	81	2·0	4·0	6·0	4·0	3·0	4·0	2·5	1·5	3·0	1·65	213·3	203	44	8·2	3·7	5·0	46	33·6	25	12·0	8·7	90·7	25·1	63·4	34·1
	1858	58·9	67·1	50·7	47·0	16·4	30·032	0·702	12	2·36	78	0·5	1·0	2·0	3·0	4·0	7·0	7·0	3·0	2·5	1·36	219·3	208	56	8·7	3·4	7·2	45	32·8	20	9·2	7·9	104·8	20·7	58·2	35·4
	1859	56·6	64·7	48·6	35·3	16·1	29·934	0·652	11	2·04	78	2·0	4·0	5·0	3·0	2·0	4·0	5·0	3·0	2·0	1·66	188·6	184	41	6·7	5·2	4·0	43	31·0	18	6·8	7·6	76·9	21·8	56·0	34·3
	1860	53·0	59·7	46·5	27·5	13·3	29·674	1·102	18	4·34	85	2·0	3·0	6·0	5·0	4·0	4·0	3·0	2·0	1·0	1·28	219·1	214	48	6·0	6·3	2·4	49	37·1	30	17·2	8·9	91·1	25·0	63·3	39·5
	1861	57·2	64·0	50·4	32·3	13·6	29·961	0·694	13	2·35	84	3·0	4·0	7·0	4·0	3·0	3·0	3·0	2·0	2·0	0·88	196·4	191	40	8·2	2·4	3·3	41	29·8	28	18·2	7·0	90·5	21·4	51·9	32·7
	1862	52·4	58·7	46·0	27·3	12·6	29·733	1·074	20	3·99	83	3·0	1·0	2·0	2·0	3·0	6·0	7·0	5·0	1·0	1·67	215·0	212	39	8·4	1·7	2·8	52	36·9	32	20·1	7·1	93·8	25·6	62·4	33·5
	Means	55·9	63·4	48·5	39·5	14·9	29·892	0·877	13·8	2·98	81·5	2·1	2·8	4·7	3·5	3·1	4·7	4·6	2·7	1·9	1·41	208·6	202·0	44·7	7·7	3·8	4·1	46·0	33·5	25·5	13·9	7·8	91·3	23·3	59·2	34·9
July	1857	58·0	65·1	51·0	46·5	14·1	29·832	0·850	15	2·17	80	1·5	1·0	0·5	0·5	2·0	9·5	10·5	3·5	2·0	1·55	210·5	202	56	8·8	3·7	12·3	47	32·4	16	5·8	6·6	100·3	20·9	56·6	32·8
	1858	56·0	63·4	48·7	44·0	14·7	29·887	0·945	17	4·31	83	2·0	2·0	2·5	2·0	2·0	6·0	6·0	4·5	4·0	1·11	224·0	214	62	8·2	3·7	11·1	45	30·1	23	9·7	9·6	111·7	24·3	57·5	30·1
	1859	59·0	66·2	51·9	32·4	14·3	30·050	1·083	14	2·76	81	1·0	2·0	4·0	2·0	2·0	7·0	8·0	2·0	3·0	1·44	181·3	175	43	5·8	6·1	7·2	42	29·6	13	6·0	5·9	84·6	18·9	48·9	29·1
	1860	57·3	64·5	50·1	30·9	14·5	29·988	0·745	11	1·82	83	3·0	2·0	4·0	3·0	3·0	3·0	6·0	4·0	3·0	0·87	214·8	210	51	5·0	7·2	6·4	45	31·3	27	14·2	8·9	101·8	24·7	58·4	29·9
	1861	56·8	63·6	50·0	27·6	13·6	29·619	0·777	20	3·94	84	1·0	2·0	3·0	3·0	4·0	7·0	7·0	2·0	2·0	1·41	204·4	200	47	6·0	3·6	6·5	45	30·7	22	11·5	7·2	95·3	22·9	53·2	32·8
	1862	53·8	60·4	47·3	26·2	13·1	29·735	0·790	22	3·87	83	1·0	1·0	2·0	2·0	3·0	7·0	9·0	5·0	1·0	1·76	192·3	189	45	10·2	2·0	3·8	39	27·9	26	15·7	5·5	83·1	23·6	53·4	32·3
	Means	56·8	63·8	49·8	34·6	14·0	29·851	0·865	16·5	3·14	82·3	1·6	1·7	2·7	2·1	2·7	6·6	7·7	3·5	2·5	1·36	204·5	198·3	50·7	7·3	4·4	7·9	43·9	30·3	21·2	10·5	7·3	96·1	22·5	54·7	31·2
August	1857	60·0	67·1	53·0	53·0	14·1	30·014	0·661	10	1·87	85	2·3	3·0	3·5	3·5	3·0	5·5	4·5	2·0	4·0	1·04	224·1	214	65	7·7	7·0	17·9	43	27·9	14	5·6	5·3	114·5	19·4	59·3	31·0
	1858	57·9	65·9	49·9	52·0	16·0	29·943	1·003	15	2·61	81	2·0	1·5	2·0	3·5	3·5	6·5	5·0	3·0	4·0	0·99	199·5	190	56	7·0	4·9	9·6	39	23·5	18	7·8	6·6	105·0	16·8	47·0	30·2
	1859	57·8	65·2	50·4	30·7	14·7	29·850	1·167	14	2·27	80	0·0	0·0	1·0	1·0	5·0	10·0	9·0	2·0	2·0	1·58	178·7	171	44	5·8	7·8	8·3	38	25·3	13	5·9	5·6	82·0	20·5	49·1	27·4
	1860	54·4	60·8	48·0	27·1	12·8	29·575	1·080	19	3·79	85	2·0	1·0	3·0	3·0	2·0	5·0	8·0	4·0	3·0	1·15	192·4	190	46	4·9	9·5	6·1	41	29·0	18	9·3	6·0	86·4	22·1	53·8	29·8
	1861	57·4	62·8	51·9	25·1	11·0	29·774	0·797	22	6·44	86	1·0	0·0	1·0	1·0	4·0	10·0	10·0	3·0	1·0	1·92	173·1	170	38	6·6	2·8	5·6	39	26·8	17	10·4	3·6	72·5	19·3	54·3	27·0
	1862	56·0	62·3	49·7	26·4	12·6	29·885	0·944	16	3·35	87	2·0	2·0	3·0	3·0	3·0	7·0	5·0	2·0	1·0	0·89	169·4	163	36	6·6	2·0	5·9	35	25·4	21	13·5	5·2	74·5	19·9	47·0	27·1
	Means	57·2	64·0	50·5	35·7	13·5	29·840	0·942	16·0	3·39	84·0	1·5	1·2	2·2	2·5	3·4	7·3	6·9	2·7	3·0	1·25	189·4	183·3	47·5	6·5	5·8	8·5	39·2	26·0	16·9	8·7	5·4	89·1	19·7	51·7	28·7
	1857	56·1	62·5	49·7	47·9	12·8	29·882	1·101	15	3·82	87	1·5	3·5	3·5	2·0	3·5	6·0	5·5	1·5	3·0	1·38	210·8	204	72	8·9	6·6	25·9	37	23·8	13	5·4	5·4	111·0	19·1	51·3	30·0
	1858	53·6	60·3	47·2	45·0	13·1	29·893	0·997	14	2·80	84	0·5	0·5	2·0	2·5	2·5	10·0	7·0	2·5	2·5	1·68	194·6	186	61	7·3	11·3	11·8	36	22·5	21	10·1	8·8	105·4	19·7	46·6	22·4

	1859	43·0	48·5	37·6	33·2	10·9	29·707	1·270	11	4·18	85	2·0	1·0	1·0	1·0	2·0	8·0	10·0	5·0	1·0	3·19	232·0	224	57	8·8	11·2	3·3	44	31·6	27	14·8	7·1	108·4	21·6	66·3	36·0
	1860	38·4	44·0	32·5	27·0	11·5	29·639	1·931	19	3·52	86	3·0	2·0	1·0	2·0	3·0	6·0	8·0	5·0	1·0	1·95	283·2	276	57	9·2	7·7	2·5	52	39·1	63	41·2	15·0	131·2	28·9	76·2	48·4
March	1861	41·1	46·9	35·5	24·2	11·4	29·507	1·430	22	5·10	86	1·0	1·0	1·0	2·0	3·0	8·0	9·0	5·0	1·0	2·64	220·2	215	48	8·4	6·1	1·8	47	35·3	37	23·1	9·3	102·4	24·0	60·2	33·4
	1862	37·8	42·5	33·2	34·8	9·3	29·698	1·322	16	3·63	88	3·0	6·0	9·0	4·0	2·0	3·0	2·0	1·0	1·0	1·36	256·7	252	58	10·1	4·7	4·0	48	36·4	48	31·8	9·0	115·5	24·7	72·1	44·8
	Means	39·8	45·2	34·5	37·3	10·7	29·701	1·593	16·1	3·55	86·0	2·2	2·6	3·6	2·2	2·2	6·1	6·9	4·2	1·1	2·15	249·8	241·6	55·3	9·9	7·6	3·6	47·8	35·1	42·2	25·2	10·5	113·9	24·3	70·2	41·5
	1857	42·7	48·7	36·7	46·0	12·0	29·767	1·142	16	2·38	84	2·0	3·0	7·0	4·0	3·0	3·5	3·5	2·0	2·0	1·18	243·6	234	60	13·8	7·3	4·9	51	37·2	29	13·4	10·7	108·6	24·1	70·6	41·4
	1858	43·8	51·4	36·1	61·0	15·3	29·942	1·434	9	1·86	78	2·0	2·0	5·0	4·0	2·0	5·0	4·5	2·5	3·0	1·45	227·1	214	45	9·0	3·9	3·0	46	34·5	32	15·5	10·6	96·5	25·1	69·1	36·1
	1859	41·3	48·7	35·2	39·0	13·5	29·751	1·289	15	3·20	80	4·0	3·0	5·0	2·0	1·0	3·0	4·0	5·0	3·0	2·33	222·7	213	50	7·5	7·0	2·5	47	33·8	26	13·3	8·4	97·0	23·5	62·0	38·0
	1860	41·5	48·9	34·0	32·2	14·9	29·978	1·860	10	1·18	82	4·0	4·0	5·0	3·0	2·0	3·0	3·0	3·0	2·0	1·55	290·2	284	57	8·4	5·0	3·7	51	38·1	66	43·9	13·3	130·4	26·8	77·5	55·2
April	1861	45·4	52·4	38·3	34·0	14·1	30·177	0·734	9	1·04	83	3·0	3·0	5·0	3·0	2·0	3·0	4·0	4·0	3·0	1·13	223·5	219	40	7·4	4·4	2·5	53	36·6	37	22·7	9·7	96·9	26·3	62·8	37·9
	1862	44·6	51·5	37·8	39·7	13·7	29·881	1·204	14	2·99	83	3·0	2·0	2·0	3·0	4·0	7·0	5·0	3·0	1·0	1·57	250·0	246	50	8·7	2·8	2·8	50	35·1	45	28·2	10·1	115·6	25·0	65·5	44·5
	Means	43·2	50·3	36·3	41·9	13·9	29·916	1·277	12·2	2·11	81·7	3·0	2·8	4·8	3·1	2·3	4·1	4·0	3·2	2·3	1·53	242·8	235·0	50·3	9·1	5·1	3·2	49·7	35·9	39·2	22·8	10·4	107·5	25·1	67·9	42·1
	1857	49·8	57·0	42·6	47·0	14·4	29·959	0·890	13	1·66	80	1·5	4·0	8·5	4·5	3·0	4·5	2·5	0·5	2·0	1·79	219·3	210	48	9·0	6·5	5·0	45	31·4	27	11·7	11·1	100·2	22·6	61·0	35·3
	1858	49·5	56·9	42·1	54·7	14·8	29·823	1·339	16	2·81	81	2·5	3·0	3·0	3·0	2·5	6·5	5·0	3·0	2·5	1·34	210·4	192	43	8·1	2·5	3·7	41	28·8	26	11·4	9·1	95·8	22·3	58·0	33·9
	1859	51·9	61·7	42·1	42·4	19·7	30·046	0·498	4	0·29	73	2·0	3·0	6·0	5·0	4·0	4·0	2·0	1·0	4·0	0·84	193·5	187	45	8·8	5·3	1·1	43	31·9	22	10·0	7·4	83·1	23·4	56·8	30·5
	1860	50·2	57·7	42·6	35·5	15·0	29·831	1·228	15	2·18	81	1·0	2·0	5·0	4·0	4·0	5·0	6·0	2·0	2·0	1·42	240·0	236	45	8·1	5·8	3·4	53	39·5	41	26·8	10·9	103·5	23·3	70·3	42·9
May	1861	49·1	56·9	41·3	39·0	15·6	30·070	0·937	14	1·57	80	4·0	3·0	3·0	1·0	1·0	6·0	7·0	4·0	2·0	1·18	225·4	218	46	11·4	2·6	2·7	47	34·6	36	22·1	9·6	102·2	25·1	65·3	32·7
	1862	51·1	58·1	44·0	31·2	14·1	29·810	0·845	16	3·89	84	1·0	2·0	3·0	4·0	5·0	6·0	6·0	2·0	2·0	1·27	228·4	224	46	9·1	2·8	4·0	50	35·5	34	19·8	8·9	98·5	26·0	67·6	36·0
	Means	50·3	58·0	42·4	41·6	15·6	29·923	0·956	13·0	2·07	79·8	2·0	2·8	4·7	3·6	3·2	5·3	4·7	2·1	2·4	1·31	219·5	211·2	45·0	9·1	4·2	3·3	46·5	33·6	31·0	16·9	9·5	97·2	23·8	63·2	35·2
	1857	57·4	66·0	48·8	66·5	17·2	30·020	1·039	9	2·79	81	2·0	4·0	6·0	4·0	3·0	4·0	2·5	1·5	3·0	1·65	213·3	203	44	8·2	3·7	5·0	46	33·6	25	12·0	8·7	90·7	25·1	63·4	34·1
	1858	58·9	67·1	50·7	47·0	16·4	30·032	0·702	12	2·36	78	0·5	1·0	2·0	3·0	4·0	7·0	7·0	3·0	2·5	1·36	219·3	208	56	8·7	3·4	7·2	45	32·8	20	9·2	7·9	104·8	20·7	58·2	35·4
	1859	56·6	64·7	48·6	35·3	16·1	29·934	0·652	11	2·04	78	2·0	4·0	5·0	3·0	2·0	4·0	5·0	3·0	2·0	1·66	188·6	184	41	6·7	5·2	4·0	43	31·0	18	6·8	7·6	76·9	21·8	56·0	34·3
	1860	53·0	59·7	46·5	27·5	13·3	29·674	1·102	18	4·34	85	2·0	3·0	6·0	5·0	4·0	4·0	3·0	2·0	1·0	1·28	219·1	214	48	6·0	6·3	2·4	49	37·1	30	17·2	8·9	91·1	25·0	63·3	39·5
June	1861	57·2	64·0	50·4	32·3	13·6	29·961	0·694	13	2·35	84	3·0	4·0	7·0	4·0	3·0	3·0	3·0	2·0	2·0	0·83	196·4	191	40	8·2	2·4	3·3	41	29·3	28	18·2	7·0	90·5	21·4	51·9	32·7
	1862	52·4	58·7	46·0	27·3	12·6	29·733	1·074	20	3·99	83	3·0	1·0	2·0	2·0	3·0	6·0	7·0	5·0	1·0	1·67	215·0	212	39	8·4	1·7	2·8	52	36·9	32	20·1	7·1	93·8	25·6	62·4	33·5
	Means	55·9	63·4	48·5	39·5	14·9	29·892	0·877	13·8	2·98	81·5	2·1	2·8	4·7	3·5	3·1	4·7	4·6	2·7	1·9	1·41	205·6	202·0	44·7	7·7	3·8	4·1	46·0	33·5	25·5	13·9	7·8	91·3	23·3	59·2	34·9
	1857	58·0	65·1	51·0	46·5	14·1	29·832	0·850	15	2·17	80	1·5	1·0	0·5	0·5	2·0	9·5	10·5	3·5	2·0	1·55	210·5	202	56	8·8	3·7	12·3	47	32·4	16	5·8	6·6	100·3	20·9	56·6	32·8
	1858	56·0	63·4	48·7	44·0	14·7	29·887	0·945	17	4·31	83	2·0	2·0	2·5	2·0	2·0	6·0	6·0	4·5	4·0	1·11	224·0	214	62	8·2	3·7	11·1	45	30·1	23	9·7	9·6	111·7	24·3	57·5	30·1
	1859	59·0	66·2	51·9	32·4	14·3	30·050	1·083	14	2·76	81	1·0	2·0	4·0	2·0	2·0	7·0	8·0	2·0	3·0	1·44	181·3	175	43	5·8	6·1	7·2	42	29·6	13	6·0	5·9	84·6	18·9	48·9	29·1
	1860	57·3	64·5	50·1	30·9	14·5	29·988	0·745	11	1·82	83	3·0	2·0	4·0	3·0	3·0	3·0	6·0	4·0	3·0	0·87	214·8	210	51	5·0	7·2	6·4	45	31·3	27	14·2	8·9	101·8	24·7	58·4	29·9
July	1861	56·8	63·6	50·0	27·6	13·6	29·619	0·777	20	3·94	84	1·0	2·0	3·0	3·0	4·0	7·0	7·0	2·0	2·0	1·41	204·4	200	47	6·0	3·6	6·5	45	30·7	22	11·5	7·2	95·3	22·9	53·2	32·8
	1862	53·8	60·4	47·3	26·2	13·1	29·735	0·790	22	3·87	83	1·0	1·0	2·0	2·0	3·0	7·0	9·0	5·0	1·0	1·76	192·3	189	45	10·2	2·0	3·8	39	27·9	26	15·7	5·5	83·1	23·6	53·4	32·3
	Means	56·8	63·8	49·8	34·6	14·0	29·851	0·865	16·5	3·14	82·3	1·6	1·7	2·7	2·1	2·7	6·6	7·7	3·5	2·5	1·36	204·5	198·3	50·7	7·3	4·4	7·9	43·9	30·3	21·2	10·5	7·3	96·1	22·5	54·7	31·2
	1857	60·0	67·1	53·0	53·0	14·1	30·014	0·661	10	1·87	85	2·3	3·0	3·5	3·5	3·0	5·5	4·5	2·0	4·0	1·04	224·1	214	65	7·7	7·0	17·9	43	27·9	14	5·6	5·3	114·5	19·4	59·3	31·0
	1858	57·9	65·9	49·9	52·0	16·0	29·943	1·003	15	2·61	81	2·0	1·5	2·0	3·5	3·5	6·5	5·0	3·0	4·0	0·99	199·5	190	56	7·0	4·9	9·6	39	23·5	18	7·8	6·6	105·0	16·8	47·0	30·2
	1859	57·8	65·2	50·4	30·7	14·7	29·850	1·167	14	2·27	80	0·0	0·0	1·0	1·0	5·0	10·0	9·0	2·0	2·0	1·58	178·7	171	44	5·8	7·8	8·3	38	25·3	13	5·9	5·6	82·0	20·5	49·1	27·4
August	1860	54·4	60·8	48·0	27·1	12·8	29·575	1·080	19	3·79	85	2·0	1·0	3·0	3·0	2·0	5·0	8·0	4·0	3·0	1·15	192·4	190	46	4·9	9·5	6·1	41	29·0	18	9·3	6·0	86·4	22·1	53·8	29·8
	1861	57·4	62·8	51·9	25·1	11·0	29·774	0·797	22	6·44	86	1·0	0·0	1·0	1·0	4·0	10·0	10·0	3·0	1·0	1·92	173·1	170	38	6·6	2·8	5·6	39	26·8	17	10·4	3·6	72·5	19·3	54·3	27·0
	1862	56·0	62·3	49·7	26·4	12·6	29·885	0·944	16	3·35	87	2·0	2·0	3·0	3·0	3·0	7·0	5·0	2·0	1·0	0·88	160·4	165	36	6·8	2·6	5·0	35	25·4	21	13·5	5·2	74·5	19·9	47·0	27·1
	Means	57·2	64·0	50·5	35·7	13·5	29·840	0·942	16·0	3·39	84·0	1·5	1·2	2·2	2·5	3·4	7·3	6·9	2·7	3·0	1·25	189·4	183·3	47·5	6·5	5·8	8·5	39·2	26·0	16·9	8·7	5·4	89·1	19·7	51·7	28·7
	1857	56·1	62·5	49·7	47·9	12·8	29·882	1·101	15	3·82	87	1·5	3·5	3·5	2·0	3·5	6·0	5·5	1·5	3·0	1·38	210·8	204	72	8·9	6·6	25·9	37	23·8	13	5·4	5·4	111·0	19·1	51·3	30·0
	1858	54·5	61·8	47·2	55·0	14·6	29·898	0·997	14	2·80	84	0·5	0·5	2·0	2·5	2·5	10·0	7·0	2·5	2·5	1·68	194·6	186	61	7·3	11·3	11·8	36	22·5	21	10·1	8·8	105·4	19·7	46·6	22·4
	1859	52·3	59·2	45·5	28·2	13·6	29·722	1·063	17	3·21	82	0·0	1·0	2·0	3·0	4·0	8·0	8·0	2·0	2·0	1·80	177·7	171	56	7·2	15·1	8·7	31	22·1	14	6·8	3·5	80·0	21·2	48·5	28·5
September	1860	50·2	57·1	43·3	35·0	13·8	29·868	1·165	14	1·92	87	3·0	2·0	2·0	1·0	2·0	6·0	8·0	4·0	2·0	1·09	182·5	179	56	5·0	18·6	6·1	34	22·4	15	8·9	3·6	90·7	20·9	47·0	23·9
	1861	53·7	59·2	48·2	27·7	11·0	29·723	1·172	19	5·27	88	3·0	1·0	2·0	3·0	4·0	6·0	6·0	3·0	2·0	1·38	165·0	162	36	5·7	3·4	5·2	31	21·4	18	11·2	5·5	74·2	16·8	45·0	28·6
	1862	53·4	60·3	46·4	32·9	14·1	29·979	0·993	13	2·44	88	1·0	2·0	3·0	4·0	3·0	6·0	5·0	2·0	4·0	0·92	195·9	193	47	7·2	3·0	6·0	39	26·5	22	15·0	5·0	91·7	24·4	53·0	26·9
	Means	52·5	60·0	46·7	37·8	13·3	29·845	1·082	15·3	3·24	86·0	1·5	1·7	2·4	2·6	3·1	7·0	6·6	2·5	2·6	1·37	187·7	182·5	54·7	6·9	9·7	10·6	34·7	23·1	17·2	9·5	5·3	92·2	20·3	48·6	26·7
	1857	49·6	55·3	43·9	47·0	11·4	29·808	1·246	16	2·36	90	1·5	3·0	3·5	3·5	3·0	7·0	6·0	1·5	2·0	1·29	208·0	198	62	13·0	8·2	14·0	35	24·0	18	8·6	5·6	93·6	22·9	57·4	34·0
	1858	44·9	51·0	39·2	32·9	12·4	29·892	1·687	18	4·72	87	2·2	2·8	2·1	1·2	1·5	7·5	7·0	4·0	2·7	1·90	205·3	193	59	7·2	17·5	5·2	33	22·2	29	12·1	12·9	111·1	19·7	47·9	26·0
	1859	45·8	51·8	39·8	43·8	12·0	29·667	1·236	14	4·60	89	3·0	3·0	4·0	4·0	2·0	4·0	4·0	4·0	3·0	1·63	188·0	181	62	6·2	27·4	5·4	30	22·0	17	6·0	5·8	87·4	25·4	48·6	26·7
October	1860	46·0	51·4	40·5	29·6	10·9	29·784	1·261	22	5·14	87	2·0	1·0	1·0	2·0	4·0	8·0	8·0	3·0	2·0	2·34	210·4	205	70	7·2	30·4	4·8	35	24·4	23	11·7	8·5	101·2	25·6	54·9	27·3
	1861	49·5	55·4	43·7	30·9	11·6	29·936	1·201	14	3·34	90	1·0	1·0	2·0	4·0	6·0	8·0	5·0	1·0	3·0	1·28	173·1	170	42	6·9	6·2	5·0	31	22·0	24	16·1	5·2	78·7	17·8	48·7	27·5
	1862	47·1	53·4	40·7	32·8	12·7	29·620	1·872	19	6·32	88	1·0	0·0	1·0	3·0	3·0	9·0	9·0	3·0	2·0	2·07	204·4	200	59	8·7	4·0	5·8	34	23·2	29	19·8	5·1	105·2	17·7	48·8	31·3
	Means	47·2	53·0	41·3	36·2	11·8	29·784	1·417	17·2	4·41	88·5	1·8	1·8	2·3	3·0	3·3	7·3	6·5	2·7	2·5	1·75	198·2	191 2	59·0	8·2	15·6	6·7	33·0	22·9	23·3	12·4	7·2	96·2	21·5	51·0	28·8
	1857	43·7	48·3	39·2	50·0	9·1	30·115	1·719	12	2·89	90	2·5	3·5	4·5	3·5	2·0	5·0	3·0	2·5	3·5	1·10	231·3	220	55	13·8	6·9	6·0	40	27·0	33	18·2	8·8	101·4	20·1	61·9	47·7
	1858	39·4	44·4	34·4	33·3	9·7	29·956	1·872	10	2·38	89	3·1	2·6	4·3	3·9	2·4	3·1	4·0	2·6	3·5	1·09	266·1	253	76	6·9	29·6	4·8	39	28·8	52	29·2	14·5	134·3	27·8	60·8	43 0
	1859	39·4	44·5	34·2	26·9	10·4	29·855	2·129	14	3·37	89	3·0	1·0	2·0	3·0	3·0	6·0	6·0	3·0	3·0	1·31	236·0	229	70	8·5	26·8	3·3	36	28·0	33	6·8	9·4	107·8	23·6	63·4	41·5
	1860	39·1	43·1	35·1	26·5	8·1	29·919	1·641	17	2·83	90	2·0	3·0	8·0	6·0	2·0	2·0	2·0	2·0	3·0	1·09	230·0	222	71	6·1	27·0	2·3	38	27·2	34	21·7	9·2	109·3	25·9	62·9	33·7
November	1861	38·5	43·9	33·0	33·4	10·9	29·544	1·295	19	6·63	87	4·0	3·0	2·0	1·0	2·0	7·0	6·0	3·0	2·0	2·36	224·5	220	54	10·9	7·7	2·9	36	25·3	42	28·8	8·5	101·7	17·7	60·2	44·6
	1862	37·1	42·6	31·6	33·3	11·0	29·897	1·513	10	2·41	90	2·0	1·0	2·0	4·0	4·0	6·0	4·0	3·0	4·0	0·80	234·7	232	69	8·5	7·0	3·4	33	24·3	46	31·7	7·9	114·3	21·1	62·4	37·1
	Means	39·5	44·4	34·6	33·9	9·9	29·881	1·695	13·7	3·42	89·2	2·8	2·3	3·8	3·6	2·6	4·9	4·2	2·6	3·2	1·29	237·1	229·3	65·8	9·1	17·5	3·8	37·0	26·7	40·0	22·7	9·7	111·5	22·4	61·9	41·3
	1857	44·9	49·2	40·7	38·0	8·5	29·989	1·338	14	3·37	88	0·5	0·0	0·0	1·0	3·0	15·0	8·0	2·0	1·5	2·32	215·7	205	43	12·0	4·4	5·0	40	28·4	35	16·4	11·3	94·0	19·4	59·4	44·0
	1858	39·9	44·0	35·7	25·2	8·3	29·703	1·297	17	4·00	90	0·8	0·4	1·2	4·1	4·8	8·9	6·4	1·9	2·2	1·80	250·0	238	77	7·9	26·6	2·8	40	29·3	39	21·4	11·5	122·3	27·9	62·4	36·8
	1859	34·0	38·3	29·7	35·5	8·7	29·651	1·853	15	3·66	88	4·0	2·0	2·0	3·0	3·0	6·0	4·0	4·0	3·0	1·67	263·4	259	69	8·5	18·1	2·6	42	31·7	51	28·8	12·1	114·4	23·2	77·3	49·0
December	1860	34·1	37·9	30·0	40·8	8·1	29·709	1·426	17	3·91	90	4·0	3·0	4·0	7·0	3·0	2·0	2·0	3·0	3·0	1·37	257·7	250	71	7·9	20·0	2·4	40	29·2	49	26·0	12·8	121·3	22·6	71·1	42·6
	1861	38·0	42·8	33·3	30·6	9·5	30·020	1·459	13	2·98	90	3·0	2·0	1·0	2·0	3·0	7·0	6·0	3·0	4·0	1·20	241·4	237	52	9·2	5·8	2·9	35	25·4	56	35·1	11·4	107·5	19·7	69·8	44·1
	1862	41·8	46·2	37·4	22·7	8·7	29·767	1·204	21	5·20	89	1·0	0·0	1·0	3·0	3·0	9·0	9·0	4·0	1·0	2·15	259·0	255	76	9·5	5·8	4·1	37	24·7	52	34·0	10·5	124·0	23·5	67·2	44·7
	Means	38·8	43·1	34·5	32·1	8·6	29·806	1·429	16·2	3·85	89·2	2·2	1·2	1·5	3·3	3·3	7 9	5·9	2·9	2·4	1·75	247·9	240·7	64·7	9·2	13·4	3·3	39·0	28·1	47·0	26·9	11·6	113·9	22·7	67·8	43·5

XXVI.—*On the Anatomical Type of Structure of the Human Umbilical Cord and Placenta.* By J. Y. Simpson, M.D., Professor of Medicine and Midwifery in the University of Edinburgh.

(Read 6th April 1863.)

In the construction of the animal kingdom, Nature shows herself always provident and saving, both in the amount of organic matter which she uses, and in the complexity of the organic structures which she moulds out of that matter. She never employs any superfluous quantity of material, nor any superfluous quality of mechanism. If a low type of structure in an organ is sufficient for the due performance of the given function of that organ, she never resorts to a higher or more complex type. She does not build organs or animals with the higher organic types of nerves, capillaries, lymphatics, &c., when these organs or animals do not require for their function, or for their life, the special physiological action of nerves, capillaries, or lymphatics. She expends no unnecessary workmanship upon the vital machinery which she employs, nor does she ever add to it any unnecessary pieces of organic apparatus.

The object of the present communication is to show, that in the human subject we have, and that too upon a large scale, a striking and remarkable instance of this great general law, though the example I allude to has never heretofore, as far as I know, been presented in this light, by any of our manifold writers on anatomy and physiology.

From an early period of utero-gestation in mammalia, the system of the fœtus is organically connected with the system of the mother by the interposition of the placenta and umbilical cord. The remarks which I wish to make upon the type of structure of these interposing organs apply to these organs themselves, as seen in all mammalia. The illustration of the observations which I desire to offer, is much more perfect in some of the lower animals—such as the cow, sheep, mare, cat, &c., which have the *placenta fœtalis* quite distinct throughout from the *placenta maternalis*—than it is in the human subject, where those two parts are more intimately and organically conjoined. I shall content myself, however, with drawing my evidence principally or entirely, at present, from human anatomy.

The human mother and the human fœtus may be looked upon as in themselves two of the most highly organised beings in existence. Yet during all the latter period of utero-gestation, their two systems are organically tied and

united together by structures of the lowest type of zoological organisation. That the structures which connect the mother and child in utero, viz. the umbilical cord and placenta, are of such a low type of organisation as I allude to, the following observations will tend to show.

Let us begin with the type of structure of the umbilical cord.

The rope-like formation constituting the *funis umbilicalis* is generally stated to consist in the latter periods of pregnancy of the following component parts, viz.:—

1*st*, Of one umbilical vein.

2*d*, Of two umbilical arteries.

3*d*, Of cellular or areolar tissue surrounding these vascular tubes, and containing in its meshes the so-called gelatine of Wharton; and

4*th*, Of a single enclosing sheath, formed by an extension of the amnion.

At an early period of pregnancy, other structures are included in the umbilical cord, as the vitellary and allantoid ducts, and the omphalo-mesenteric vessels; but these are not connected with our inquiry, and nothing, at the most, but very shrivelled remnants of them remain in the fully-developed cord.

In relation to the subject of our present inquiry, it is of more importance to mark the interesting fact, that though by many of our greatest and best anatomists the finest injections have been often thrown into the umbilical vessels, both arteries and veins, no capillaries have ever thus, or otherwise, been traced in any of the component tissues of these tubes. No *vasa vasorum* have ever been detected in the walls or structures of the umbilical arteries or veins. On this point, one of the latest and most careful anatomists who has written on the "Formation and Circulation of the Human Placenta," viz., Professor SCHROEDER VAN DER KOLK of Utrecht, observes as follows:—"That the umbilical cord has no smaller capillaries besides the large umbilical vessels, is universally accepted. Whether this opinion be grounded on a careful examination, I know not; but it appeared to me sufficiently important to make it an object of particular inquiry. By filling with sufficient force the umbilical vessels with very fine injection matter, so that the *vasa vasorum*, if any be present, must also be filled, I saw, after the cord was dried, moistened with turpentine, and examined under the microscope, that nowhere was there a trace of any *vasa vasorum*, so that the umbilical cord contains the only example of blood-vessels which receive no *vasa vasorum*. After an examination for this very purpose, I could as little discover them in umbilical cords in the third month of pregnancy as in others after pregnancy had been perfect."

The statement that the umbilical cord contains in its structure no capillary vessels, or even any *vasa vasorum*, requires to be qualified with one remark, viz., that the capillaries and *vasa vasorum*, which within the body of the fœtus extend along the umbilical vessels and abdominal walls up to the umbilical ring, or true

fœtal end of the cord, pass there onwards, according to the dissections and injections of SCHOTT and VAN DER KOLK, for two or three lines, but they do not stretch for any further distance along the track of the cord itself. They exist, in other words, in the short persistent end of the cord, which remains attached after birth to the umbilicus, and which ultimately fills up the umbilical ring, but they are not found in that great mass which forms the deciduous part of the cord, running from the child to the placenta.

The same observation holds true, according to SCHOTT and VALENTIN, regarding the short extension of the terminal twigs of the nerves of the fœtus, which pass out of the body of the fœtus along with the umbilical vessels through the umbilical ring. Apparently, like the capillaries, they do not pass further than one or two lines beyond that ring. Numerous attempts have been made to trace nerves in the course of the umbilical cord, but no continuous line of nerve tissue has been detected in its track. In former times, CHAUSSIER and BAER fancied that they had found nervous fibres running along it. Anatomists generally hold that these supposed nerves were only the deceptive remains of the obliterated vitelline duct or omphalo-mesenteric vessels. At all events, since the microscope has come to add its great and necessary aid to this inquiry, the search has been diligently renewed, but hitherto in vain. No nervous fibril, or continuation of fibrils, has been detected by it in this structure.

Several years ago, my nephew, Dr ALEXANDER SIMPSON, when writing his Thesis on the Umbilical Cord, set himself assiduously, with the scalpel and microscope, to the investigation of this question of the presence or absence of nerves in the cord. These investigations were kindly overlooked by Professor GOODSIR. The result was, that not a trace of a nervous fibril could be detected in this structure.

In some observations which I published twelve years ago, on the Contractility of the Umbilical Vessels, I attempted to show that we could generally, after the child is born, produce in the umbilical arteries and veins, by the local application of mechanical, chemical, or electrical stimulants, local contractions, which did not again relax. But this form of contractility does not prove, I believe, that nervous fibres exist in the walls of the umbilical arteries and veins; for such local phenomena of contractility occur in the lower animals, where no nerve fibres have been found to exist, as in the Actinia, Medusa, &c. Let me add, that one great exceptional peculiarity in the low type of structure of the umbilical cord is the presence of strong and well-marked contractile muscular fibres in the walls of its vessels, though these fibres are totally unprovided with nerves.

Lymphatics have been alleged to exist in the cord by various authors, as WRISBERG, SCHRŒGER, and MICHAELIS, and especially by FOHMAN. The umbilical lymphatics were supposed by FOHMAN to be capable of being injected by quick-

silver; and plates of their appearance, when thus injected, were published by him. But it is now, I believe, universally allowed that these mercurial injections were thrown into the areolæ of the cellular tissue of the cord, and not into lymphatic vessels; and I know of no anatomist of the present day who gives credit to the existence of lymphatics in that structure.

What forms the proper volume of the cord, or, in other words, what forms the material which surrounds the umbilical vessels, and fills up the serous-like sheath of the cord, is usually spoken of as cellular tissue, containing in its delicate meshes a hyaline substance of an albuminoid or mucoid nature, termed the "Gelatine of Wharton." This cellular tissue closely resembles ordinary connective tissue, and is composed of nucleated stellate cells or corpuscles.

The external covering of the cord consists of a serous-like blue membrane or sheath, which, after covering the placenta, envelops the cord from the placenta onwards to the umbilical ring, where, by an abrupt line of division, it meets the white skin of the abdomen of the full-grown fœtus. At birth, the contrast at their line of union of the white opaque skin of the fœtus with the blue semi-transparent covering of the cord is very striking, and gives in itself the impression of a human cutaneous surface connected organically with a structure of a low zoological type.

The umbilical cord, as an organic medium of communication between the fœtus and mother, is merely as it were a sheath of sarcoid matter perforated with three tubes for the transmission of the fœtal blood to and from the placenta; but it contains apparently no lymphatics, no nerves, no capillaries, not even *vasa vasorum*. It is skinless, and contained within a sheath of serous membrane. It consists essentially of large nucleated cells formed partly into large and loose cellular tissue containing the gelatine of Wharton, and developed partly into tubal forms, constituting the so-called umbilical arteries and veins.

The same remarks which apply to the type of structure of the umbilical cord apply to the type of structure of the placenta.

Into the maternal surface of the placenta no anatomist has hitherto traced the passage of any nervous branches from the applied surface of the uterus, nor have any nutrient arteries been as yet, at least, shown to pass from the uterus into the maternal substance of the human placenta. But at all events, the fœtal portion, which remains throughout in some animals distinct from the maternal, is assuredly of the same type of structure as the cord, with one or two points of difference; for, 1*st*, it contains no gelatine of Wharton; and, 2*d*, its vessels, both arteries and veins, divide and sub-divide, till they reach the very small size in which they appear in the terminal villi; but still neither their coats nor the surrounding tissue contain any capillaries or *vasa vasorum*, or lymphatics, or nerves.

The enveloping sheath or lining of the terminal villi contains a new arrangement of tissue not seen in the umbilical cord or other parts of the placenta.

For the duplicatures of veins and arteries contained in the terminal villi are surrounded, as was first shown by Mr Goodsir in one of his admirable papers on the placenta, by a thin layer of nucleated cells, probably, as he suggests, for the functions of nutrition and respiration. This new layer of tissue is apparently derived from the decidua or hypertrophied mucous membrane of the uterus, and, along with the lining membrane of the maternal vascular system of the placental blood-cells, constitutes the maternal portion of the human placenta; but neither nerves nor capillaries have been traced into these structures.

The mode or modes of nourishment and growth of a structure like the umbilical cord and placenta, presenting no capillaries nor *vasa vasorum*, constitutes an interesting problem in physiology; but my remarks are at present limited to peculiarities in the mere anatomical type of these parts. These anatomical peculiarities, to which I have attempted to draw the attention of the Society, may in conclusion be summed up as follows:—

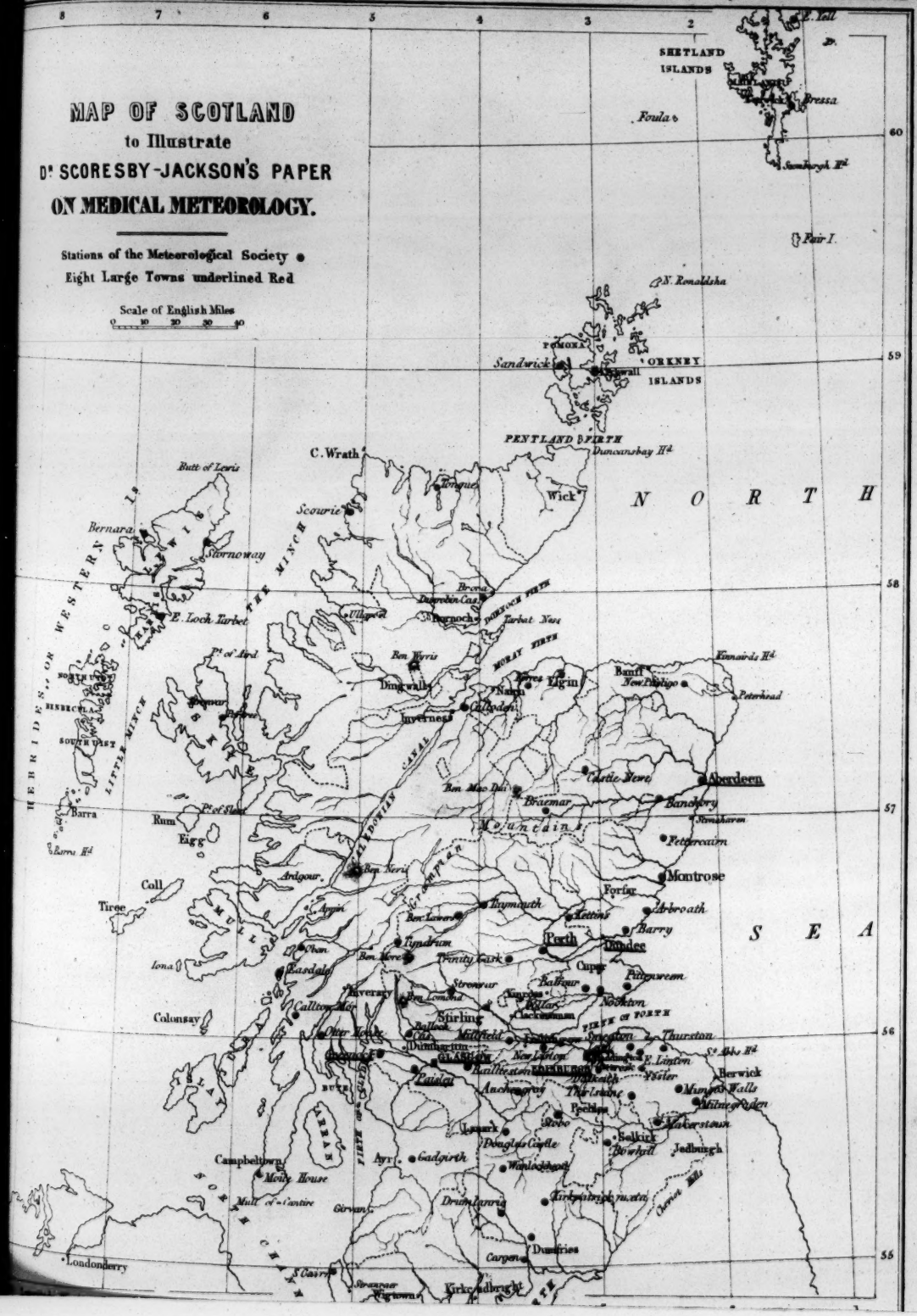
1. The volume of the umbilical cord and fœtal portion of the placenta is formed of nucleated cellular tissue, traversed by the tubes of the umbilical arteries and vein and their numerous placental subdivisions, and invested by a sheath of serous membrane.

2. Into the composition of these parts, no capillaries, *vasa vasorum*, lymphatics, nor nerves, are found to enter.

3. Hence, in human anatomy, we have these organs, forming a large mass, weighing on an average about two pounds, presenting a type of structure resembling that of some of the inferior zoophytes. And,

4. The human mother and her child, two of the most highly organised beings in existence, are thus temporarily united together, during the intra-uterine life of the latter, by structures of the lowest zoological type.

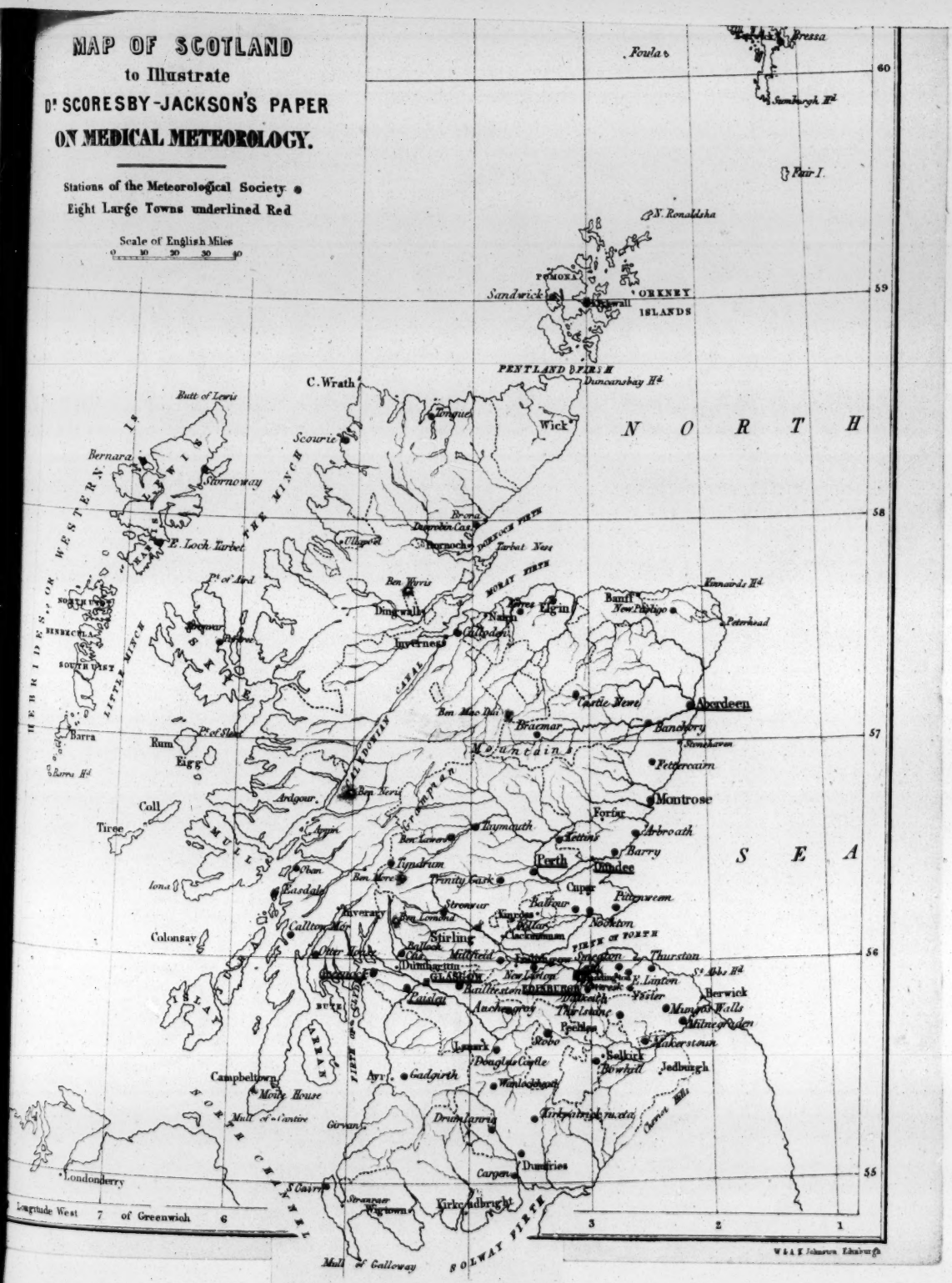

HEBRIDES


MAP OF SCOTLAND
to Illustrate
Dr SCORESBY-JACKSON'S PAPER
ON MEDICAL METEOROLOGY.
Stations of the Meteorological Society ●
Eight Large Towns underlined Red
Scale of English Miles
0 10 20 30 40
SHETLAND ISLANDS
Foula
Fair I.
N. Ronaldsha
ORKNEY ISLANDS
Sandwick
Kirkwall
PENTLAND FIRTH
Duncansbay Hd
C. Wrath
Butt of Lewis
Scourie
Tongue
Wick
Bernara
Stornoway
E. Loch Tarbet
Broras
Dornoch
Tarbat Ness
Ben Wyvis
Dingwall
Nairn
Elgin
Banff
New Pitsligo
Kinnaird's Hd
Peterhead
Inverness
Caledon
Aberdeen
Banchory
Braemar
Ben Mac Dui
Mountains
Fettercairn
Montrose
Forfar
Arbroath
Barry
Dundee
Perth
Tay
Ben Lawers
Tyndrum
Ben More
Trinity Gask
Cupar
Pittenweem
Balfour
Newton
Stirling
Clackmannan
Dunfermline
Thurston
Haddington
Berwick
Kelso
Jedburgh
Selkirk
Bowhill
Peebles
Moffat
Douglas Castle
Wanlockhead
Dumfries
Kirkcudbright
Gadgirth
Ayr
Campbeltown
Mouth of Clyde
Londonderry
Coll
Tiree
Rum
Eigg
Barra
Colonsay
Iona
Mull
Skye
Lewis
Harris
North Uist
South Uist
Benbecula
Hebrides or Western Isles
Little Minch
The Minch
Caledonian Canal
Moray Firth
Firth of Forth
Firth of Clyde
Solway Firth
Cheviot Hills
NORTH SEA
IRISH SEA
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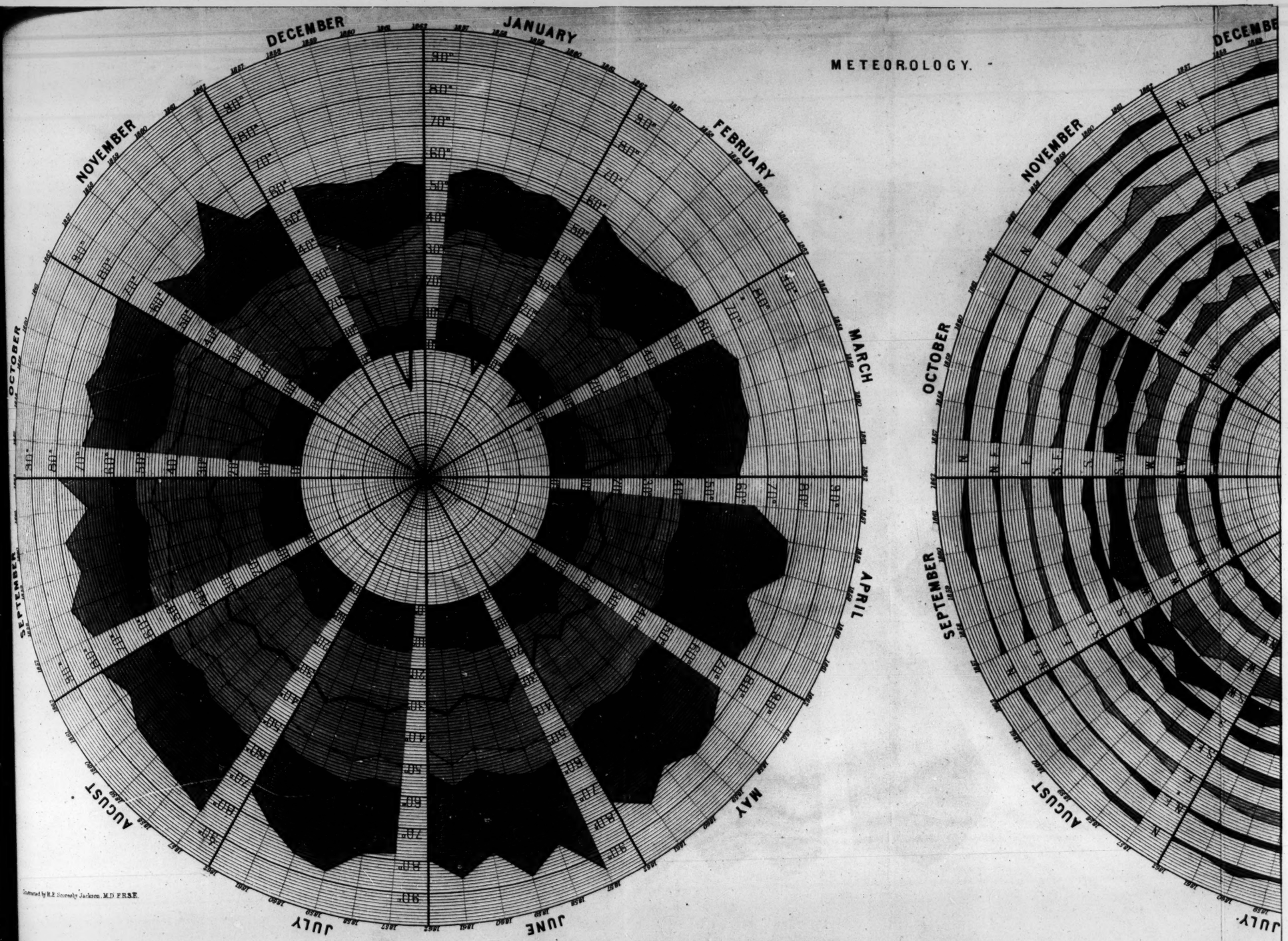
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HEBRIDES, OR

Longitude


MAP OF SCOTLAND
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Stations of the Meteorological Society ●
Eight Large Towns underlined Red
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0 10 20 30 40
Foula
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Tongue
Wick
Scourie
Bernara
Stornoway
Broora
Dunrobin Cas.
Tarbat Ness
E. Loch Tarbet
Ullapool
Ben Wyvis
Dingwall
Nairn
Forres
Elgin
Banff
New Pitsligo
Kinnaird's Hd
Peterhead
Inverness
Culloden
Fort George
Fort Augustus
Ben Mac Dui
Castle Steuart
Aberdeen
Braemar
Banchory
Stonehaven
Mountains
Pettercairn
Montrose
Forfar
Arbroath
Barry
Dundee
Perth
Ben Nevis
Ardgour
Tyndrum
Ben Lawers
Tay mouth
Kettins
Trinity Gask
Cupar
Pittenweem
Easdale
Oban
Iona
Tiree
Coll
Barra
Rum
Eigg
Colonsay
Inveraray
Loch Lomond
Kinross
Kelty
Stirling
Callander
Dunblane
Millfield
Auchterarder
Crieff
Dunkeld
Perth
Thurston
Dunbar
Haddington
E. Linton
Berwick
Musselburgh
Walls
Milne's Raden
Edinburgh
New Lebton
Bonnington
Portobello
Musselburgh
Dalkeith
Thirlstane
Peebles
Stobo
Makerstoun
Selkirk
Bowhill
Jedburgh
Douglas Castle
Wanlockhead
Gadgirth
Ayr
Campbeltown
Mealy House
Mull of Cantire
Girvan
Drumlanrig
Kirkpatrick
Dumfries
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Portpatrick
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W. & A. Johnston, Edinburgh

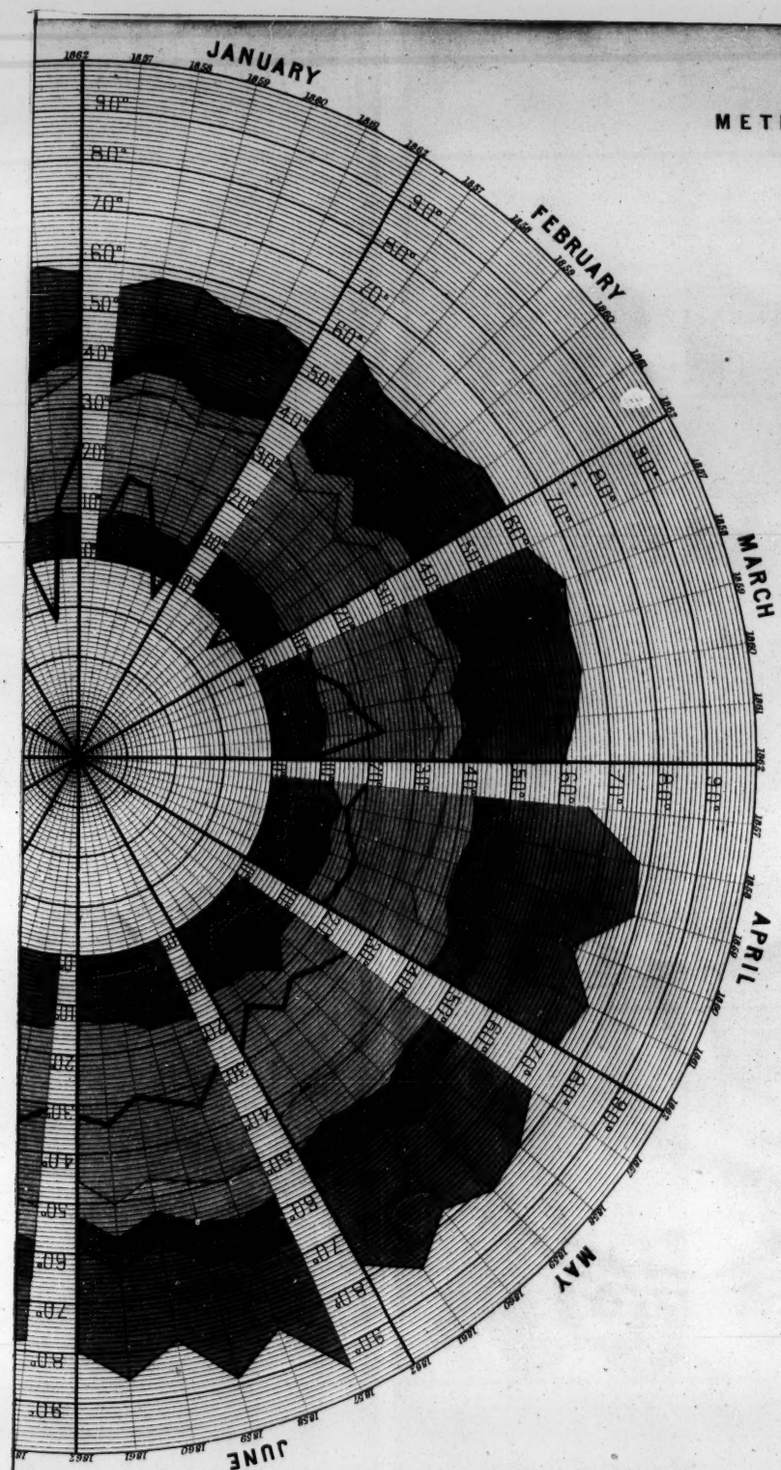
METEOROLOGY.

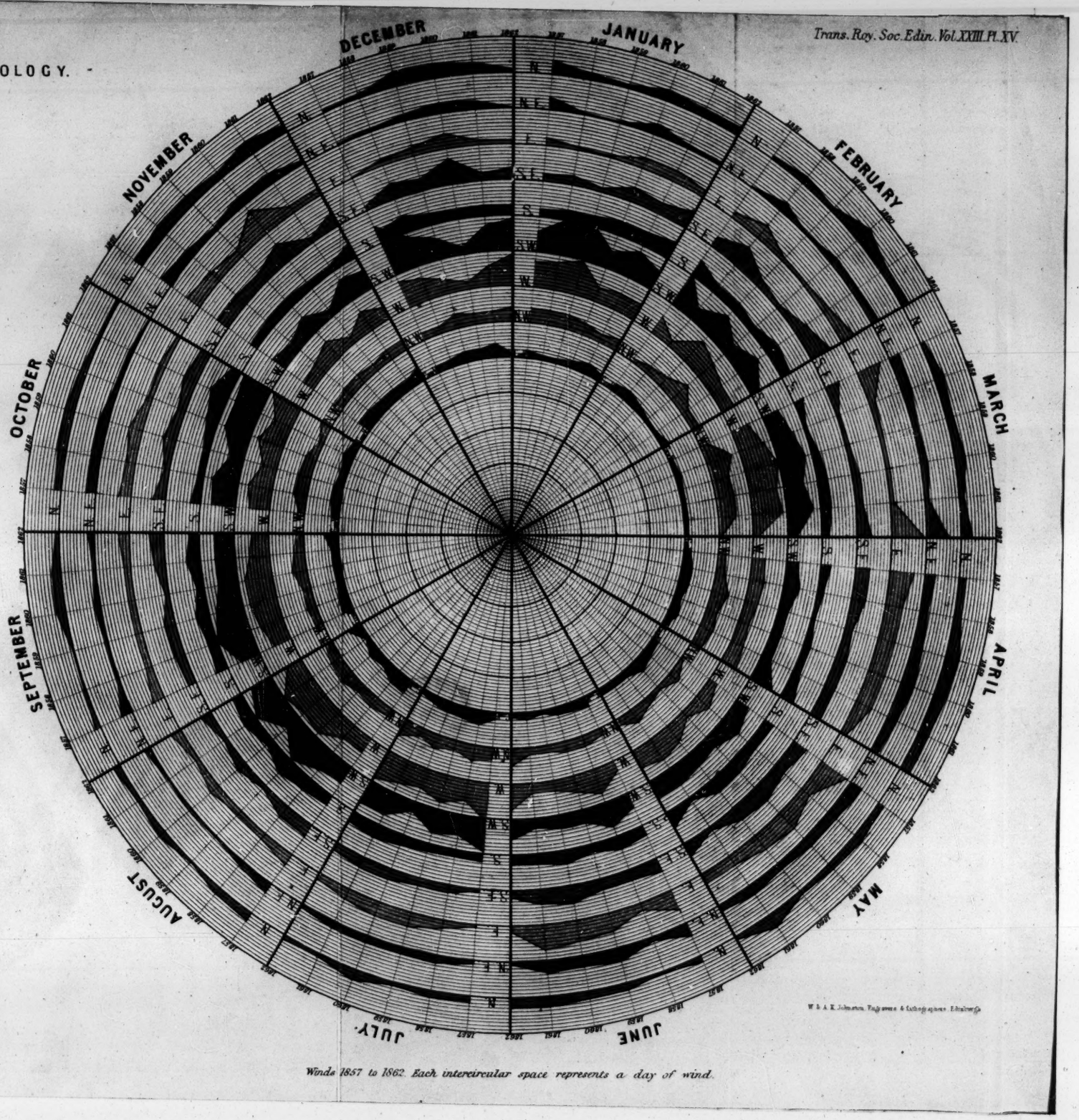


Temperature (Fahrenheit) 1857 to 1862, Absolute Highest — Mean of Day — Mean Temperature — Mean of Night — Absolute Lowest — Mean Daily Range —

Winds 1857 to 1862

Trans. Roy. Soc. Edin. Vol. XXIII. Pl. XV.

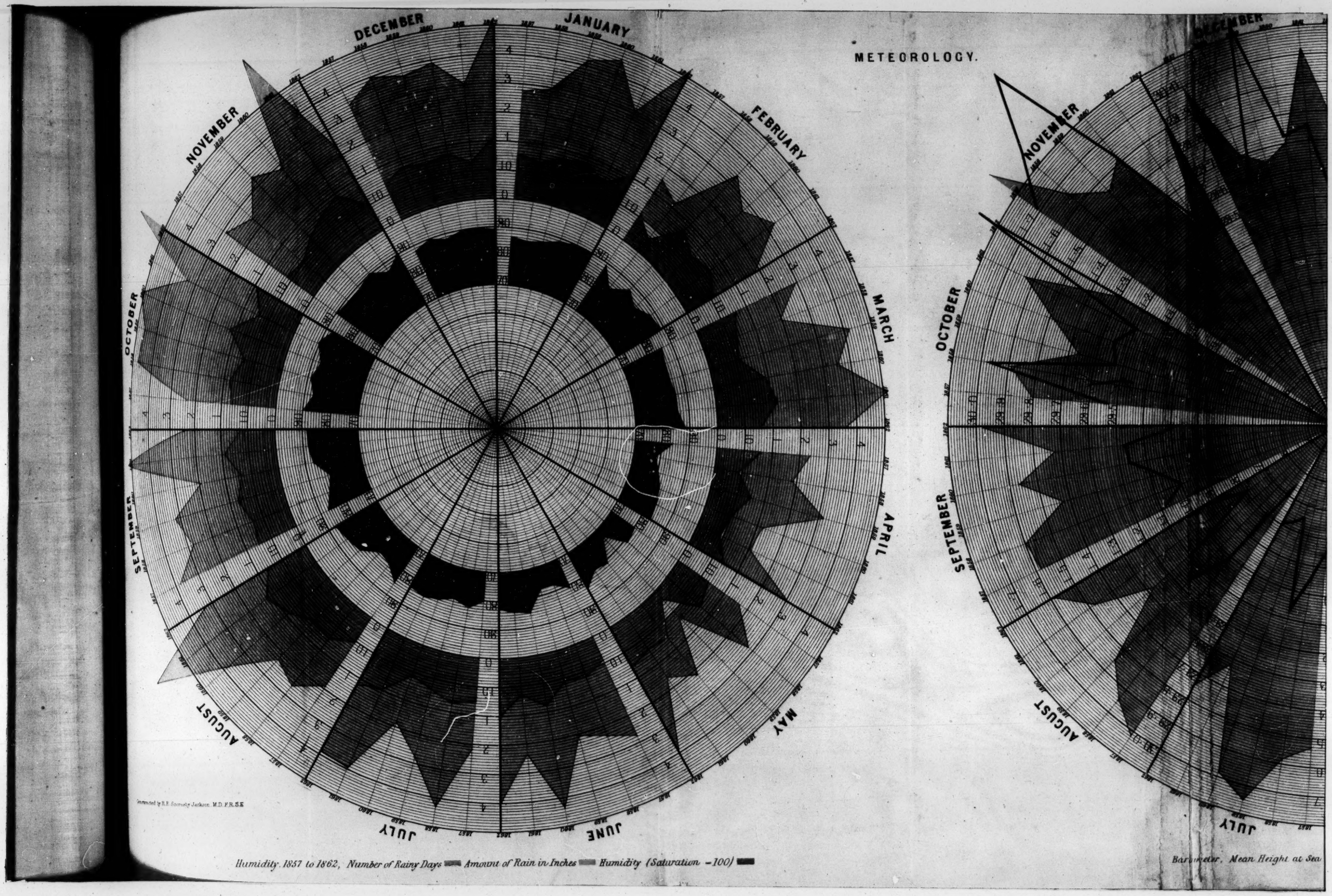




Mean Temperature *Mean of Night* *Absolute Lowest* *Mean Daily Range*

Winds 1857 to 1862. Each intercircular space represents a day of wind.

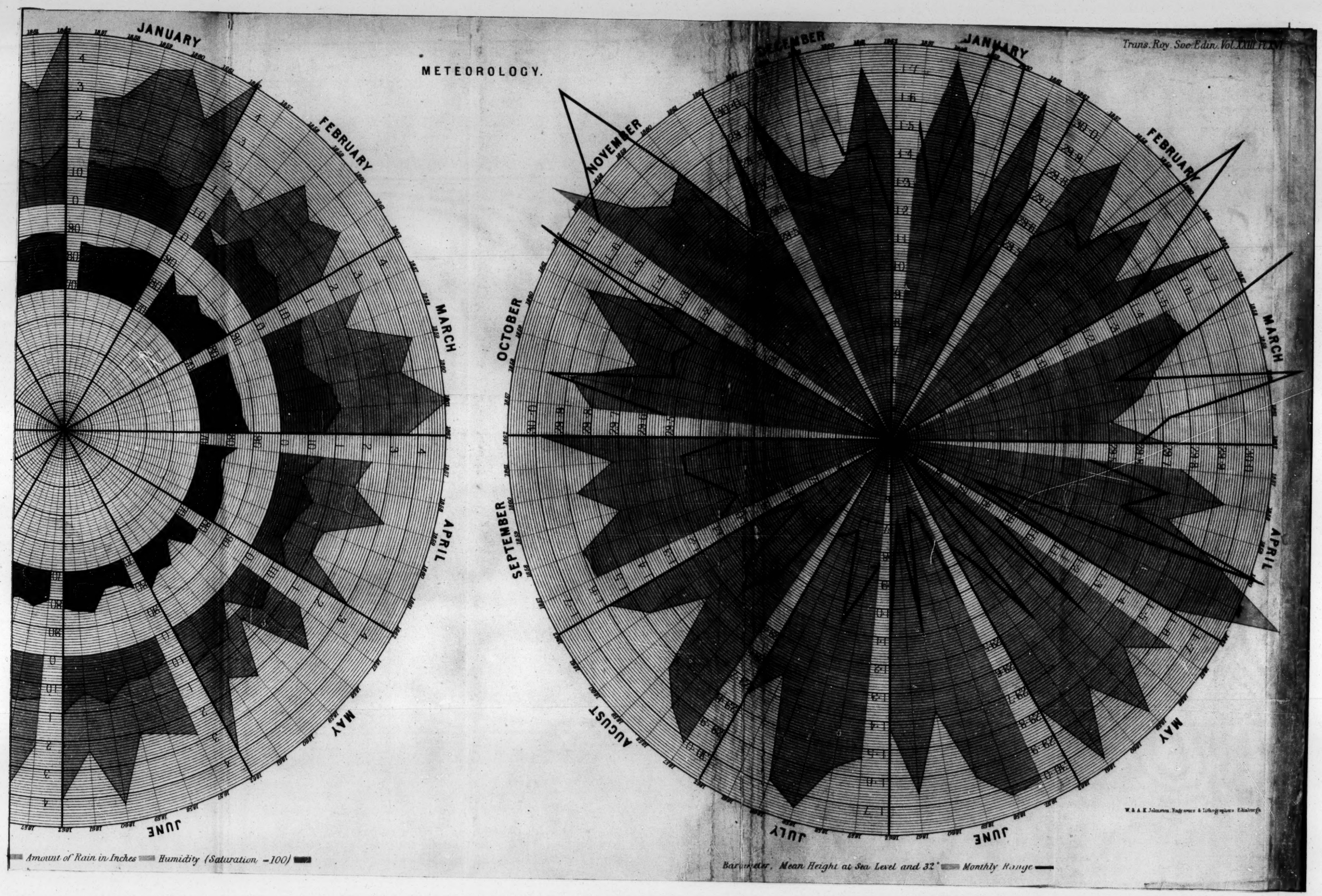
W. & A. K. Johnston, Engravers & Lithographers, Edinburgh



Humidity. 1857 to 1862, Number of Rainy Days ▬ Amount of Rain in Inches ▬ Humidity (Saturation = 100) ▬

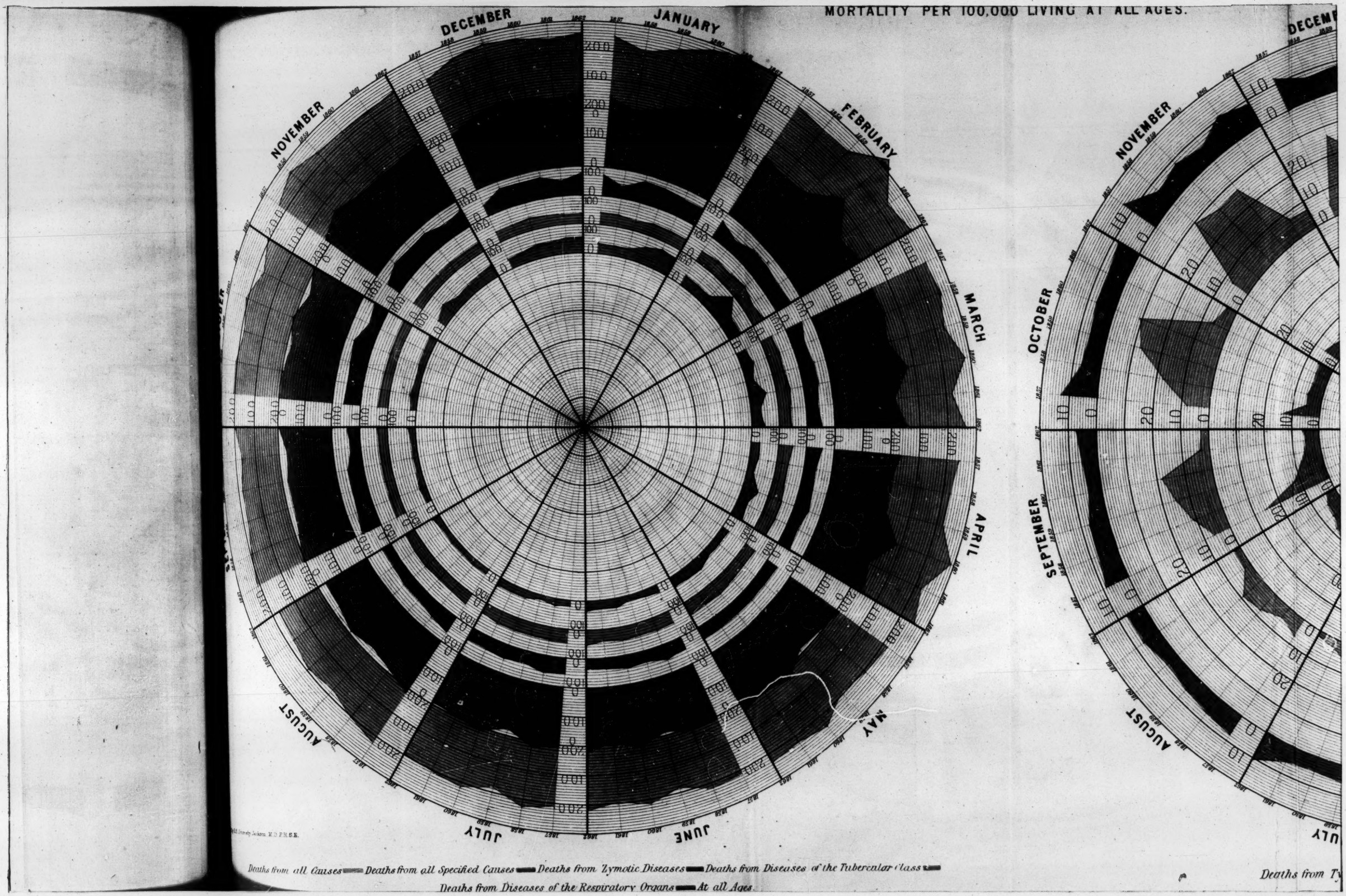
Barometer. Mean Height at Sea

METEOROLOGY.



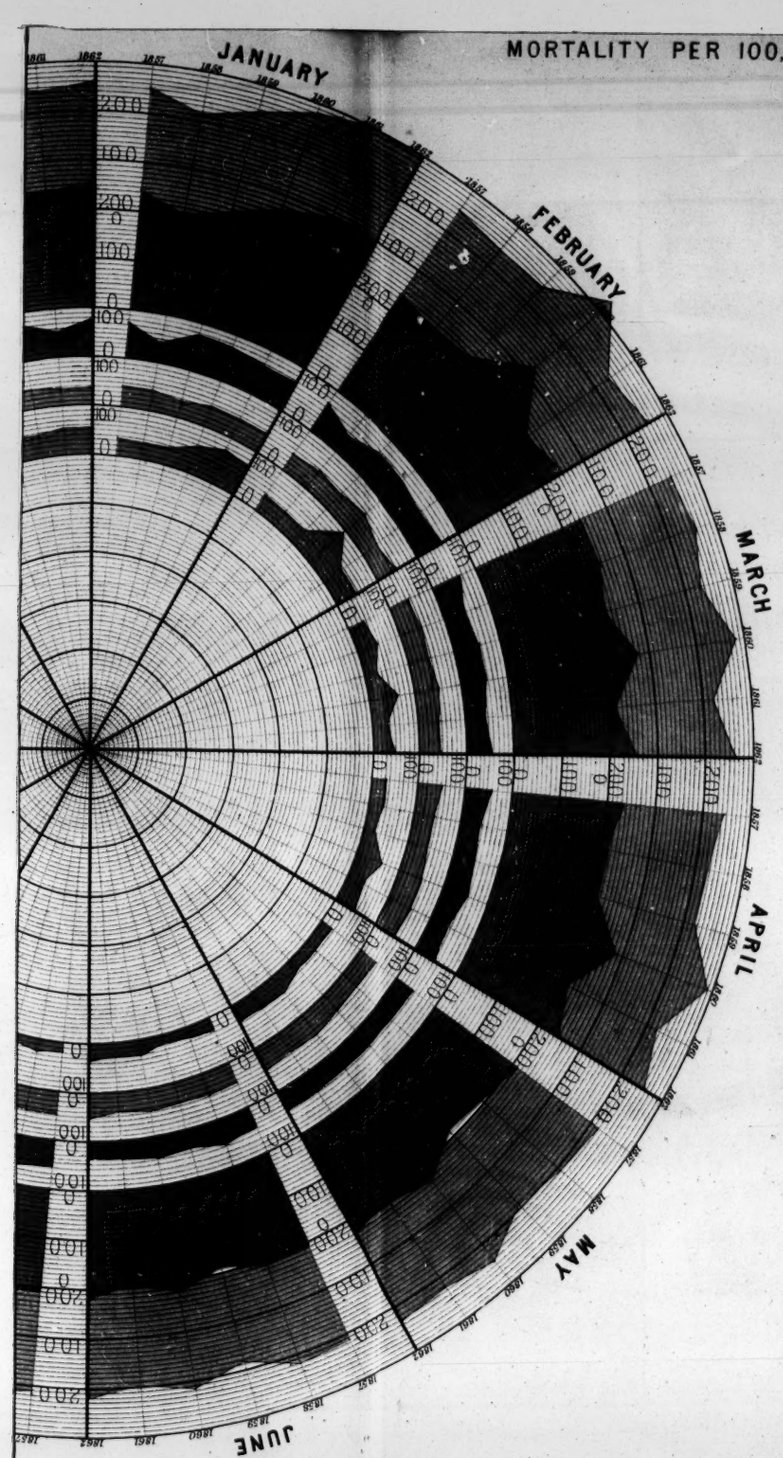
W. & A. K. Johnston, Engravers & Lithographers, Edinburgh.

MORTALITY PER 100,000 LIVING AT ALL AGES.

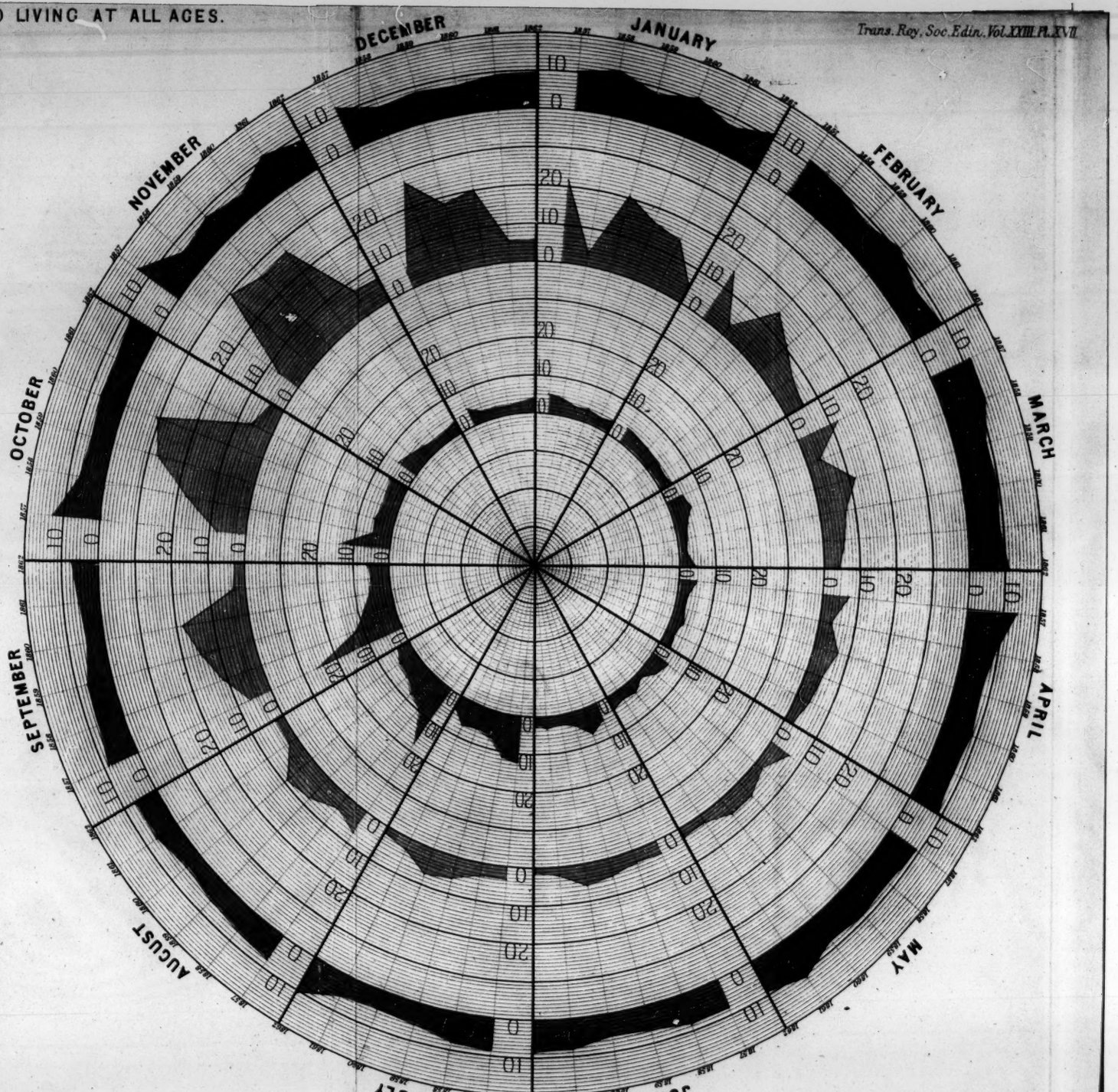


Deaths from all Causes ▬ Deaths from all Specified Causes ▬ Deaths from Zymotic Diseases ▬ Deaths from Diseases of the Tubercular Class ▬
Deaths from Diseases of the Respiratory Organs ▬ At all Ages

MORTALITY PER 100,000 LIVING AT ALL AGES.

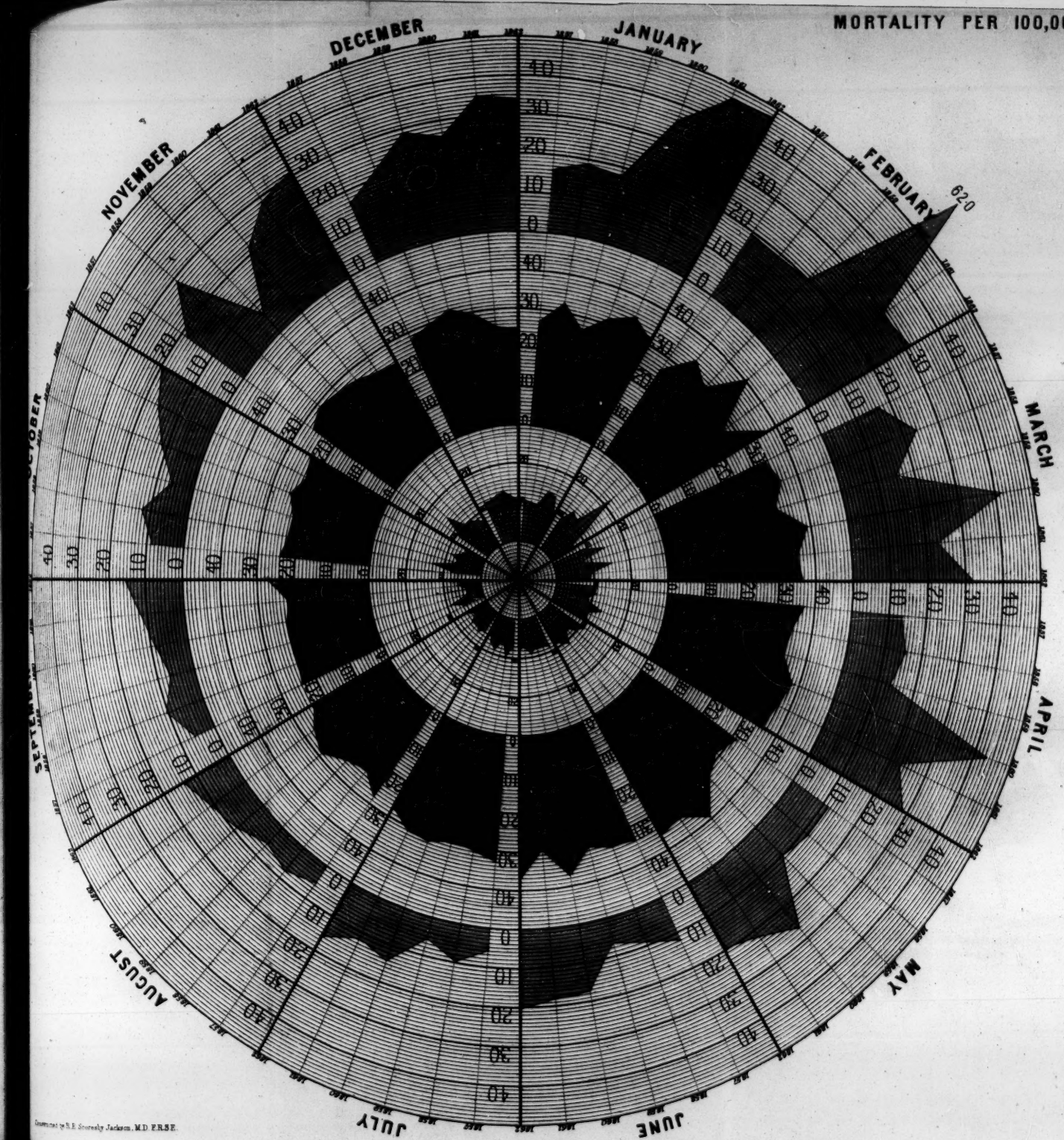


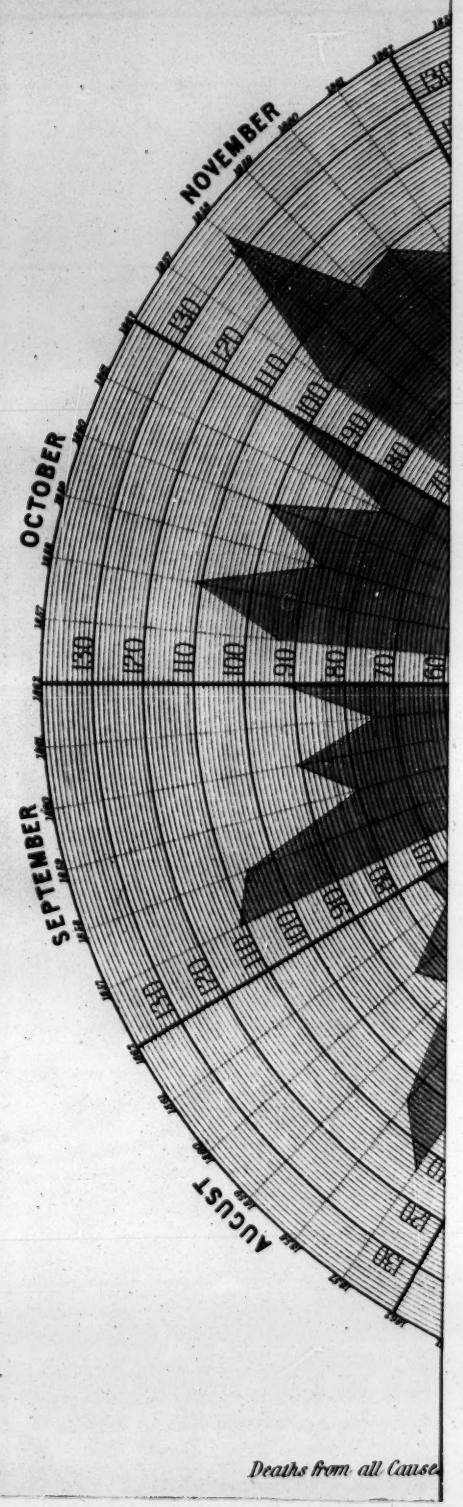
Deaths from Zymotic Diseases ▬ Deaths from Diseases of the Tubercular Class ▬



Deaths from Typhus ▬ Scarlatina ▬ and Diarrhœa ▬ at all Ages.

MORTALITY PER 100,000 LIVING AT ALL AGES.



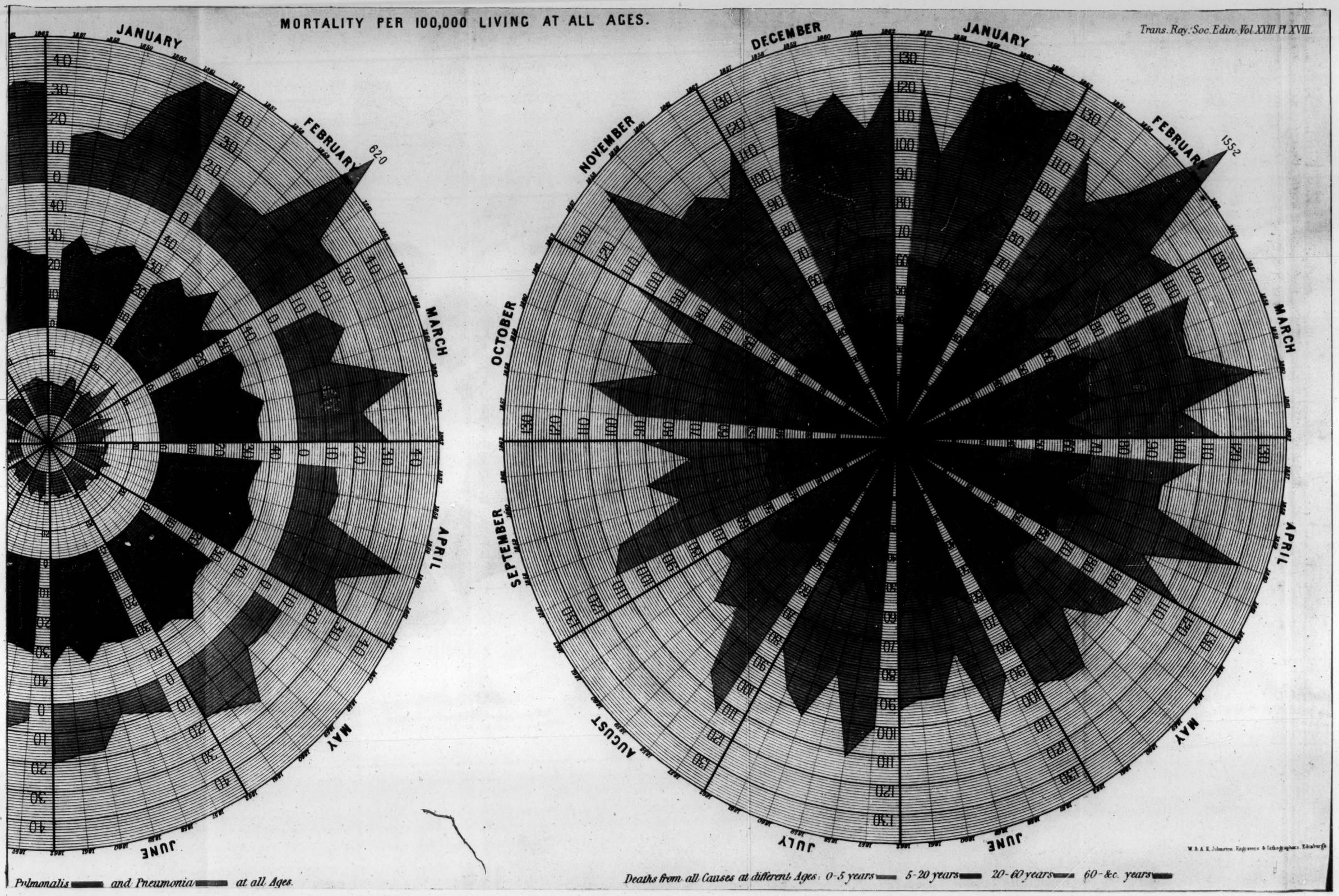


Drawn by S.E. Scoresby Jackson, M.D. F.R.S.E.

Deaths from Bronchitis ▬ *Phthisis Pulmonalis* ▬ *and Pneumonia* ▬ *at all Ages.*

Deaths from all Cause

MORTALITY PER 100,000 LIVING AT ALL AGES.



Pulmonalis ▬ *and Pneumonia* ▬ *at all Ages.*

Deaths from all Causes at different Ages: 0-5 years ▬ *5-20 years* ▬ *20-60 years* ▬ *60-&c. years* ▬

W. & A. K. Johnston, Engravers & Lithographers, Edinburgh

XXVII.—*On Earth-Currents during Magnetic Calms, and their connection with Magnetic Changes.* By Balfour Stewart, M.A., F.R.S.

(Read 6th April 1863.)

I have already endeavoured*† to prove that Auroræ and Earth-currents, far from producing magnetic disturbances by their direct action, are themselves only secondary or induced currents, generated by those small but abrupt changes of the earth's magnetism which constitute such disturbances. The proof of this statement was in the first place derived in a general manner from the fact that during the notable magnetic storm of August and September 1859, all the elements of the earth's magnetism at Kew remained for many hours on one side of their normal positions, while, on the other hand, the earth-currents observed during that time by Mr C. V. Walker had their direction reversed every two or three minutes.*

In the next place it was shown that the earth-currents, which occurred simultaneously with a singularly abrupt and isolated disturbance, recorded by photography at Kew, were of a character which would favour the induction hypothesis rather than that of direct action.†

Professor W. Thomson has likewise found by calculation, that the electromotive force, induced by variations of terrestrial magnetism, is probably comparable in amount with that which manifests itself in earth-currents.†

Since the publication of these views, Mr Walker has extended his inquiries, and has communicated to the Royal Society of London an account of some earth-currents which he had observed by the aid of a sensitive galvanometer during a period of magnetic calm.‡ It is these observations which I propose to discuss in the present communication.

Mr Walker has been extremely careful in determining that all his observed currents were in no sense atmospheric, but were true and proper earth-currents, whatever may have been their cause. In order to accomplish this, he had a telegraphic wire of 67 miles in length, connected with the earth at one of its extremities, the other being insulated. On this wire he made many observations during various hours of different days, but always failed in obtaining a current unless when both ends were connected with the earth.

* Phil. Trans. for 1861, p. 423. † Ibid. for 1862, p. 621. ‡ Ibid. for 1862, p. 203.

We are thus provided with a mass of apparently unexceptionable observations of earth-currents made during a period when the magnet was comparatively tranquil, and the question arises, What connection have these with the daily changes which take place in the magnetism of the earth?

In attempting to answer this question, it is, I think, necessary to divide the observations of earth-currents into two categories, for the following reason.

It has been shown by General SABINE, that magnetic disturbances obey laws of time and place very different from those followed by the ordinary daily magnetic changes. Now, if we consider that such disturbances, more or less violent, are of very frequent occurrence, we shall see at once the necessity, in magnetical discussions, of separating the disturbed observations from the undisturbed, in order to obtain accurately the laws of both.

Let us bear this in mind, and even without speculating on the connection between earth-currents and magnetic changes, we cannot, I think, expect that a magnetic disturbance of a certain definite type shall be connected in precisely the same way with an earth-current, as an ordinary daily magnetic change of the same type as the disturbance.

This will appear evident, if we reflect that although both changes may be of the same kind here, yet, if we alter our position and go into another portion of the globe, they may be quite different one from another, since a change of position will affect the disturbance and the daily variation according to very different laws.

On any hypothesis, therefore, the disposition over the earth's surface of that current which is connected with the disturbance, will be different from that which is connected with the ordinary daily change. For these reasons it would appear to be highly necessary to ascertain which of the earth-currents correspond in time of occurrence with disturbances of greater or less amount, and which with undisturbed observations.

I have been enabled to ascertain this, at least approximately, by means of the photographic records of the earth's magnetism at Kew. The portions of the Kew declination and horizontal force curves, corresponding in time with the observations of earth-currents, have been carefully inspected, and whenever any appearance of disturbance presented itself, this has been noted by the side of the corresponding earth-current observations, and these have by this means been divided into the three following classes, viz.:—

Class I. Observations during Magnetic Calms.
II. Observations during smaller Disturbances.
III. Observations during greater and more abrupt Disturbances.

These are recorded in the three following tables, Mr WALKER's notation being adopted:—

TABLE I.—Observations during Magnetic Calms.

Date.	Time.	Column 1. Dover—London, u.* London—Dover, d.	Column 2. Tonbridge—London, u. London—Tonbridge, d.	Column 3. Dover—Tonbridge, u. Tonbridge—Dover, d.
1861.		°	°	°
October 1	9.31 A.M.	16 u	6 d	14 u
... ...	11.42 ...	15 u	35 d	30 u
... ...	2.20 P.M.	4 u	36 d	...
... ...	2.35 ...	3 u	28 d	...
... 2	2.15 ...	17 u	35 d	20 u
... ...	2.47 ...	0	0	0
... ...	3.20 ...	6 u	28 d	18 u
... 3	5.20 ...	19 d	3 d	20 d
... ...	10.12 ...	20 u	0	20 u
... 4	6.25 A.M.	33 d	3 u	45 d
... ...	12.24 P.M.	35 u	29 d	50 u
... ...	12.26 ...	40 u	55 u	50 u
... ...	4.50 ...	10 d	20 d	2 d
... 8	11.0 A.M.	20 d	10 d	31 d
... 9	6.20 ...	12 u	20 d	25 u
... ...	9.20 ...	35 d	25 u	35 d
... 10	6.58 ...	10 u	15 d	20 u
... 14	2.16 P.M.	10 u	12 d	11 d
... ...	2.45 ...	5 u	25 d	15 u
... 15	11.34 A.M.	20 u	40 d	32 u
... ...	12.19 P.M.	18 u	16 d	25 u
... ...	2.30 ...	2 u	15 d	7 u
... ...	3.25 ...	10 d	4 u	15 d
... ...	7.16 ...	20 u & d	5 u	40 to 60 d
... 17	10.52 A.M.	23 u	10 d	30 u
... ...	11.25 ...	20 u	25 d	38 u
... ...	11.43 ...	26 u	26 d	40 u
... ...	2.13 P.M.	20 u	18 d	35 u
... ...	3.41 ...	10 u	20 d	15 u
... ...	5.6 ...	10 d	15 d	0
... 18	10.58 A.M.	14 u	10 d	28 u
... ...	11.37 ...	24 u	10 d	34 u
... ...	12.30 P.M.	40 u	30 d	48 u
... ...	2.44 ...	22 u	18 d	34 u
... ...	3.39 ...	15 u	12 d	16 u
... 19	12.1 ...	12 u	40 d	45 u
... 21	7.7 A.M.	5 d	0	35 d
... ...	10.10 ...	28 d	15 u	32 d
... ...	1.42 P.M.	50 u	10 d	50 u
... ...	2.56 ...	43 u	15 d	47 u
... 22	6 15 A.M.	0	5 d	2 u
... ...	7.15 ...	5 d	0	25 d
... 23	7.0 ...	5 d	0	0
... ...	1.45 P.M.	40 u	0	45 u
... ...	3.9 ...	45 u	0	50 u
... 24	6.20 A.M.	0	7 d	7 u
... ...	7.5 ...	0	0	2 u
... 26	12.19 P.M.	0	10 u	0
... ...	1.10 ...	20 u	20 d	26 u
... ...	4 1 ...	0	5 d	0

* Dover—London, u, means that a stream of positive electricity is travelling (to use railway language) *up* from Dover to London, and a similar rule applies to the other headings.

TABLE I.—*Continued.*

Date.	Time.	Column 1. Dover—London, u. London—Dover, d.	Column 2. Tonbridge—London, u. London—Tonbridge, d	Column 3. Dover—Tonbridge, u. Tonbridge—Dover, d.
1861.		°	°	°
October 26	10.45 P.M.	10 d	10 d	10 d
... 28	10.20 ...	5 d	10 d	10 d
... ...	10.50 ...	15 d	5 u	5 to 20 d
... ...	11.0 ...	20 d	5 to 15 u	20 d
... 29	12.10 A.M.	22 d	22 d	22 d
... ...	12.20 ...	10 d	0	15 d
... ...	12.30 ...	18 d	8 d	20 d
... ...	12.50 ...	10 d	7 d	13 d
... ...	1.0 ...	10 d	5 d	12 d
... ...	1.10 ...	15 d	6 d	20 d
... ...	1.20 ...	12 d	8 d	20 d
... ...	1.30 ...	18 d	6 d	20 d
... ...	1.40 ...	12 d	5 d	19 d
... ...	1.50 ...	15 d	5 d	21 d
... ...	2.10 ...	18 d	0	20 d
... ...	2.50 ...	5 d	0	10 d
... ...	3.0 ...	15 d	10 d	15 d
... ...	3.10 ...	13 d	5 d	19 d
... ...	3.20 ...	13 d	0	16 d
... ...	3.30 ...	15 d	0	20 d
... ...	3.40 ...	12 d	4 d	5 d
... ...	3.50 ...	10 d	0	12 d
... ...	4.0 ...	12 d	2 d	19 d
... ...	4.10 ...	14 d	10 u	20 d
... ...	4.20 ...	10 d	5 u	20 d
... ...	4.30 ...	9 d	0 to 5 u	11 d
... ...	4.43 ...	10 d	0 to 5 u	12 d
... ...	4.53 ...	10 d	0 to 5 u	15 d
... ...	5.0 ...	8 d	0 to 10 u	14 d
... ...	5.10 ...	8 d	8 u	18 d
... ...	5.40 ...	4 d	0 to 4 u	5 d
... ...	5.50 ...	0	10 u	2 d
... ...	6.0 ...	0	5 u to 5 d	0
... ...	6.10 ...	0	0 to 5 u	0
... ...	6.20 ...	0	5 d	0
... ...	6.30 ...	0	0	0
... ...	7.30 ...	15 u	0	20 u
... ...	7.47 ...	15 u	0	15 u
... ...	3.13 P.M.	10 d	12 u	16 d
... ...	3.29 ...	10 d	12 u	18 d
... 30	6.10 A.M.	0	10 u	10 d
... ...	6.30 ...	0	10 u	5 d
... ...	6.41 ...	0	20 u	8 d
... ...	7.42 ...	0	15 u	5 d
... ...	9.20 ...	17 u	1 to 10 d	20 u
... 31	6.12 ...	10 d	10 u	20 d
... ...	6.30 ...	0	10 u	0
... ...	9.43 ...	18 u	8 u to 12 d	21 u
... ...	11.59 ...	14 u	42 d	38 u
... ...	6.54 P.M.	0	0 to 15 u	4 d
Nov. 17	12.35 ...	...	10 u	28 u
... 20	7.34 A.M.	...	...	25 d

TABLE I.—*Continued.*

Date.	Time.	Column 1. Dover—London, u. London—Dover, d.	Column 2. Tonbridge—London, u. London—Tonbridge, d.	Column 3. Dover—Tonbridge, u. Tonbridge—Dover, d.
1861.		°	°	°
Nov. 20	7.37 A.M.	...	10 d	...
... ...	10.50 ...	...	8 u	...
... 21	7.7 ...	...	...	0
... ...	7.10 ...	...	5 d	...
... ...	9.35 ...	...	18 u	...
... ...	12.49 P.M.	...	0	...
... 22	7.1 A.M.	...	...	15 u
... 23	9.1 ...	..	10 u	...
... 25	1.31 P.M.	0	...	0
... ...	1.34 ...	...	12 u	...
... 27	6.17 A.M.	...	0	...
... ...	6.19 ...	...	...	10 u
... ...	11.47 ...	10 d	...	15 d
... ...	12.32 P.M.	...	5 d	16 d
... ...	2.18 ...	17 d	3 u	28 d
... 28	6.15 A.M.	0	...	0
... ...	6.17 ...	...	5 u	...
... 29	6.20 ...	0	...	5 d
... ...	6.24 ...	...	5 u	...
... ...	6.33 ...	...	...	5 d
... 30	6.21 ...	5 d	...	10 d
... ...	6.17 ...	...	10 u	...
Dec. 2	11.49 ...	23 u	8 d	34 u
... ...	2.24 P.M.	15 u	...	22 u
... 3	6.13 A.M.	...	0	18 d
... ...	6.24 ...	15 d	...	18 d
... 4	6.15 ...	5 d	...	0
... ...	6.17 ...	...	15 d	...
... 5	6.20 ...	0	...	0
... ...	6.24 ...	...	0	...
... ...	12.34 P.M.	17 u	...	40 u
... 6	6.20 A.M.	0	...	0
... 13	6.40 ...	10 u	0	10 u
... ...	6.49 ...	...	...	10 u
... 14	6.30 ...	5 d	...	15 d
... ...	6.35 ...	10 u	10 u	15 d
... 17	6.17 ...	10 d	...	10 d
... ...	6.29 ...	...	5 d	...
... ...	6.30 ...	...	...	10 d
... 18	6.18 ...	15 d	...	10 d
... ...	6.20 ...	...	10 d	...
... 21	6.18 ...	0	...	10 u
... ...	6.22 ...	...	28 d	...
... 26	6.25 ...	20 u	...	25 u
... ...	6.30 ...	...	10 d	...
... ...	6.34 ...	...	...	25 u
... 27	6.30 ...	10 u	10 d	15 u
... ...	6.34 ...	...	0	...
... 28	6.17 ...	0	...	0
... ...	6.21 ...	...	0	...
... 31	6.19 ...	25 d	...	30 d
... ...	6.23 ...	...	0	...

TABLE II.—Observations during Smaller Disturbances.

Date.	Time.	Column 1. Dover—London, u. London—Dover, d.	Column 2. Tonbridge—London, u. London—Tonbridge, d.	Column 3. Dover—Tonbridge, u. Tonbridge—Dover, d.
1861.		°	°	°
October 2	9.34 A.M.	9 u	4 d	11 u
... ...	12.24 P.M.	23 u	26 d	42 u
... 3	7.2 A.M.	35 d	5 d	20 d
... ...	11.44 ...	32 u	14 d	38 u
... 8	12.56 P.M.	20 u	15 u	15 u
... ...	1.47 ...	45 u	10 d	50 u
... 11	10.25 A.M.	0	30 u	0
... 12	7.8 ...	5 u	40 d	40 u
... ...	9.10 ...	20 d	20 u	40 d
... 14	11.59 ...	10 d	10 d	10 d
... ...	12.52 P.M.	25 u	30 d	35 u
... 15	1 23 ...	8 u	30 d	20 u
... 16	6.37 A.M.	10 d	5 u	35 d
... ...	7.44 ...	5 d	35 u	35 d
... ...	12.18 P.M.	40 u	50 d	45 u
... 18	6.35 A.M.	30 d	0	35 d
... ...	7.17 ...	35 d	25 u	45 d
... ...	8.36 ...	35 d	30 u	32 d
... ...	12.57 P.M.	22 u	15 d	30 u
... ...	1.36 ...	20 u	25 d	40 u
... 19	7.12 A.M.	25 d	10 u	38 d
... 21	1.24 P.M.	50 u	22 d	...
... ...	2.21 ...	55 u	23 d	52 u
... ...	3.35 ...	45 u	22 d	50 u
... 22	7.57 ...	5 d	0	5 d
... 23	6.13 A.M.	0	5 u	0
... ...	7.31 ...	5 d	10 u	30 d
... 24	7.47 ...	10 d	20 d	5 u
... 25	6.14 ...	15 d	5 u	30 d
... ...	10.29 ...	40 d	10 u	33 d
... ...	11.10 ...	16 d	13 u	30 d
... ...	4.50 P.M.	20 u	20 d	30 u
... ...	9.25 ...	25 d	15 u	40 d
... *26	6.10 A.M.	20 u	25 d	30 u
... ...	7.32 ...	20 d	35 u	20 d
... ...	7.44 ...	25 d	40 u	40 d
... ...	8.50 ...	0	5 d	0
... ...	10.54 ...	21 d	15 u	40 d
... ...	12.57 P.M.	0	0	0
... ...	1.26 ...	0	8 u	0
... ...	9.45 ...	0	10 d	0
... 28	10.19 A.M.	0	20 d	8 u
... ...	10.35 ...	23 d	15 u	32 d
... ...	11.2 ...	5 d	24 d	0
... ...	11.18 ...	16 d	18 d	12 d
... ...	11.37 ...	20 d	10 u	35 d
... ...	12.57 P.M.	23 d	0	26 d

* The declination needle was on this occasion considerably to the west of its normal position.

TABLE II.—*Continued.*

Date.	Time.	Column 1. Dover—London, u. London—Dover, d.	Column 2. Tonbridge—London, u. London—Tonbridge, d.	Column 3. Dover—Tonbridge, u. Tonbridge—Dover, d.
1861.		°	°	°
Oct. 28	2.50 P.M.	10 d	3 u	14 d
... ...	10.10 ...	20 u	15 d	15 u
... ...	10.30 ...	5 d	10 d	5 d
... ...	10.40 ...	5 u	25 d	20 u
... ...	11.10 ...	0	10 d	3 u
... ...	11.20 ...	15 u	18 d	20 u
... ...	11.30 ...	5 u	...	5 u
... ...	11.40 ...	36 d	42 u	52 d
... ...	11.50 ...	25 d	22 u	35 d
... ...	12.0 ...	18 d	0	20 d
... 29	12.40 A.M.	22 d	8 u	36 d
... ...	2.0 ...	18 d	10 u	13 d
... ...	2.20 ...	23 d	19 u	13 d
... ...	2.30 ...	18 d	...	20 d
... ...	2.40 ...	10 d	15 u	12 d
... ...	5.20 ...	4 d	0 to 10 u	5 d
... ...	5.30 ...	0	0 to 15 u	0
... ...	6.40 ...	5 u	0	8 u
... ...	6.50 ...	5 u	10 d	25 u
... ...	7.7 ...	10 u	7 u	10 u
... ...	12.2 P.M.	8 u	40 d	28 u
... ...	12.25 ...	0	10 d	0
Nov. 15	12.34 ...	...	18 d	...
... ...	1.56 ...	...	24 d	...
... 16	10.53 A.M.	...	50 d	...
.. 21	11.46 ...	10 d	6 u	20 d
... 23	6.22 ...	...	10 d	...
... 25	2.47 P.M.	...	5 u	...
... ...	3.23 ...	16 u	...	14 u
... ...	3.24 ...	...	23 u	...
... 26	6.34 A.M.	...	10 d	...
... ...	6.35 ...	...	...	35 u
... ...	3.24 P.M.	5 u	...	10 u
... 27	2.38 ...	18 d	...	26 d
... 29	1·13 ...	...	...	17 d
Dec. 4	12·9 ...	20 u	...	13 u
... ...	1.24 ...	34 u	...	40 u
... ...	1.29 ...	...	16 d	...
... ...	2·40 ...	20 u	...	16 u
... 6	6.39 A.M.	0	...	5 u
... 7	6.20 ...	16 u	5 d	25 u
... 9	7.12 ...	15 u	...	20 u
... ...	7·16 ...	...	0	...
... 19	6.20 ...	0	...	0
... ...	6.21 ...	...	10 d	...
... 20	6.20 ...	0	25 d	5 u

TABLE III.—Observations during Greater and more Abrupt Disturbances.

Date.	Time.	Column 1. Dover—London, u. London—Dover, d.	Column 2. Tonbridge—London, u. London—Tonbridge, d.	Column 3. Dover—Tonbridge, u. Tonbridge—Dover, d.
1861.		°	°	°
Oct. 5	7.44 A.M.	35 d	50 d	45 d
... 8	3.12 P.M.	48 u	50 d	55 u
... 10	3.14 ...	30 u	50 d	50 u
... *	10.0 ...	55 d	5 u	10 d
... 11	6.25 A.M.	55 u	50 d	55 u
... ...	9.6 ...	55 d	35 u	50 d
... 24	7.41 ...	20 d	20 u	20 u
... ...	7.54 ...	15 u	22 d	35 u
... ...	9.20 ...	45 d	25 u	50 d
... ...	9·37 ...	55 d	45 u	55 d
... ...	10.25 ...	30 d	15 u	43 d
... ...	11.24 ...	0	12 d	8 u
... ...	11.47 ...	13 d	25 u	38 d
... ...	12.29 P.M.	0	15 d	0
... ...	2.40 ...	22 u	55 u	15 u
... ..	2.44 ...	55 u	50 u	50 u
... ...	3.32 ...	55 u	13 d	55 u
... ...	4.23 ...	7 u	35 u	0
... ...	5.3 ...	3 d	32 u	40 d
... 25	6.42 A.M.	50 d	20 u	55 d
... 28	7.15 ...	0	15 u	0
... ...	12.13 P.M.	20 d	4 u	27 d
... ...	1.9 ...	30 d	14 u	40 d
Dec. 19	8.18 ...	...	...	55 d

With the exception of the two cases noted, the declination and horizontal force needles were in no instance very far from their normal positions.

Let us now return to Table I., and endeavour by means of it to find the normal values of the earth-currents for each of the three lines, corresponding to the various hours of the day. And here it is necessary to remark that the strength of the current is not strictly proportional to the angle of deflection noted in the table, but rather to its tangent, which Mr Walker has kindly informed me will, in the case of his galvanometer, approximately represent the value of the current. Taking therefore the tangents of the angles, and averaging the results for each hour, we obtain the following tables, in which the sign + denotes an up, and the sign − a down-current.

* The declination needle was on this occasion considerably to the east of its normal position.

TABLE IV.—Hourly Means of the Earth-Currents observed on the London and Dover Line during Magnetic Calms.

	0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.
Number of observations, .	12	3	9	11	3	3		1			2	3	2	5	4	4	6	5	24	13	3	2	3	4
Mean value of current, . .	+34	+50	+29	+19	+15	−23		0			+14	−27	−29	−23	−28	−20	−21	−16	−7	+3	+18	−20	+4	+17

TABLE V.—Hourly Means of the Earth-Currents observed on the London and Tonbridge Line during Magnetic Calms.

	0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.
Number of observations, .	11	5	9	10	3	3		2			2	3	2	6	3	4	6	5	23	15	4	3	3	5
Mean value of current, . .	−30	−17	−24	−16	−22	−23		+10			−9	+3	−20	−12	−6	−7	+3	+7	−3	+1	+2	+18	+16	−17

TABLE VI.—Hourly Means of the Earth-Currents observed on the Tonbridge and Dover Line during Magnetic Calms.

	0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.
Number of observations, .	14	4	7	11	3	3		2			3	2	3	4	4	4	6	5	26	18	4	2	4	4
Mean value of current, . .	+68	+39	+32	+26	+18	−13		−63			0	−29	−34	−29	−36	−27	−29	−25	−8	−2	+2	−17	+7	+32

It will be seen that each of these tables exhibits a daily period, and that in each the values of the currents for the hours of the day are greater than those for the hours of the night.

Also we may regard the means for 0^h, 2^h, 3^h, 18^h, 19^h, as being best determined.

Let us employ the following method as perhaps the best in our power for exhibiting the characteristic difference between the three classes of earth-currents. Let us represent in a tabular form the departures from the means above given of the individual observations of each of these classes.

TABLE VII.—Departures from the Means in Table IV. of the Individual Earth-Currents observed on the London and Dover Line.

Class I.—*Observations during Magnetic Calms.*

0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.
− 7	+ 34	− 22	− 14	+ 3	−11		0			+ 22	+ 9	−11	− 9	− 4	+11	− 6	0	− 58	+ 15	+ 9	− 50	+ 25	−53
+36	− 14	+ 1	− 19	+ 12	+ 5					− 23	0	+11	+ 5	+ 7	− 7	0	− 2	+ 28	− 12	+ 9	+ 50	− 57	+26
+50	− 20	− 11	− 9	− 15	+ 5						− 9		+ 5	+ 1	− 3	+ 4	− 2	+ 7	− 12	− 18		+ 28	+19
+ 2		− 26	− 10										− 4	− 4	− 3	0	+ 2	+ 7	− 11				+ 8
− 2		+ 55	− 16										+ 2			− 4	+ 2	0	− 3				
+15		+ 90	− 37													+ 4		+ 7	− 3				
+10		− 29	+ 21															+ 7	− 3				
−13		− 59	+ 74															+ 7	− 3				
−34		− 2	+ 81															+ 7	− 3				
− 9			− 37															+ 7	+ 15				
−52			− 37															− 10	− 11				
+ 8																		+ 7	+ 14				
																		+ 7	+ 14				
																		− 2					
																		− 20					
																		− 2					
																		+ 7					
																		+ 7					
																		− 11					
																		− 20					
																		+ 7					
																		+ 43					
																		+ 7					
																		− 40					

Class II.—*Observations during smaller Disturbances.*

+ 8	− 14	+ 71	− 37	+ 85	+59					− 14	+18	+38	−17	− 4	−12		+ 9	+ 7	− 73	− 27	− 16	+ 12	−46
+38	− 3	+ 7	+ 10							+ 12	+35	−44		−14	+ 2			− 20	+ 6	− 27	− 50	− 4	−55
−52	− 36	+114	− 10								+27	−18						*+ 43	− 21	− 36	+ 20	− 80	−59
+50	− 10		− 51								+54	− 3						+ 7	− 73	− 54		− 4	−58
−70	+ 69		+ 7															+ 35	− 61	− 64			−46
−20	− 50																	+ 7	− 50				
−34	− 50																	+ 7	+ 6				
−52	− 92																		+ 6				
+ 2	+ 17																		+ 15				
																			− 3				
																			+ 24				

Class III.—*Observations during greater and more Abrupt Disturbances.*

−57	−108		+ 92	+128	+18					†−157								+150	−122	− 88	−123	−147	−17
−34			+ 39	− 3															− 3	− 54	− 80	− 62	
−70			+ 21																	+ 9			
			+124																				

* The declination needle was on this occasion considerably to the west of its normal position.
† The declination needle was on this occasion considerably to the east of its normal position.

TABLE VIII.—Departures from the Means in Table V. of the Individual Earth-Currents observed on the London and Tonbridge Line.

Class I.—*Observations during Magnetic Calms.*

0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.	12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.
- 40	- 41	- 49	- 37	- 14	+ 18		- 1			+ 9	- 21	- 20	- 2	- 3	+ 7	- 3	- 4	+ 8	- 28	- 2	+ 29	- 26	0
- 25	- 19	- 46	+ 16	+ 1	- 13		+ 2			- 9	+ 6	+ 20	0	- 3	- 11	- 10	- 4	- 33	- 1	- 2	- 27	+ 11	0
+ 173	+ 35	+ 3	- 37	+ 14	- 4						+ 15		+ 3	+ 6	- 2	- 3	- 3	- 6	- 1	+ 25	- 1	+ 16	- 30
- 54	+ 17	- 3	- 31										+ 2		+ 7	- 6	+ 2	- 9	- 1	- 20			0
+ 1	+ 8	- 8	+ 23										- 2			+ 15	+ 7	+ 6	- 1				+ 31
- 19		+ 6	- 16										+ 1			+ 6		+ 21	- 1				
+ 12		+ 24	- 11															+ 3	+ 17				
- 54		+ 45	+ 16															+ 6	+ 35				
+ 48		+ 29	+ 37															- 6	+ 17				
- 60			+ 37															+ 21	- 10				
+ 16																		+ 20	- 1				
																		+ 3	+ 17				
																		+ 12	- 19				
																		+ 12	- 19				
																		+ 20	- 1				
																		+ 3					
																		- 24					
																		+ 3					
																		- 6					
																		- 15					
																		- 50					
																		+ 3					
																		+ 3					

Class II.—*Observations during smaller Disturbances.*

- 19	+ 44	+ 6	+ 21	- 18	- 13					- 9	- 21	+ 110	+ 23	+ 23	+ 34		+ 2	+ 12	- 10	+ 68	+ 18	- 23	+ 44
+ 5	- 41	- 23	+ 25							- 18	- 50	+ 60		+ 40				+ 12	- 85	+ 16	+ 40	+ 42	- 27
+ 12	- 41	- 18	+ 58								- 21	+ 20						* - 44	+ 8	- 38	- 27	+ 2	- 15
- 89	- 10	- 20									- 35							+ 15	- 1	+ 68		- 52	- 102
+ 48	- 23																	- 14	+ 46	+ 82			+ 44
- 54	+ 17																	- 6	+ 17				
+ 12	+ 31																	- 14	- 1				
+ 40	+ 17																	- 44	- 19				
	- 15																		+ 11				
	- 12																		- 19				
																			- 1				

Class III.—*Observations during greater and more Abrupt Disturbances.*

- 3	+ 42		- 103	- 1	+ 85					† + 18								- 116	+ 35	- 121	+ 52	+ 84	- 4
+ 37			- 103	+ 92															+ 26	+ 34	+ 29	+ 11	
+ 77			+ 159																	- 42			
			+ 135																				

* The declination needle was on this occasion considerably to the west of its normal position.
† The declination needle was on this occasion considerably to the east of its normal position.

TABLE IX.—Departures from the Means in Table VI. of the Individual Earth-Currents observed on the Tonbridge and Dover Line.

Class I.—*Observations during Magnetic Calms.*

0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	*10h.	11h.	12h.	13h.	14h.	15h	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.
− 10	+ 10	+ 4	− 14	+ 9	−23		−56			+ 36	+ 8	− 6	+ 6	0	+ 9	− 7	+ 6	− 92	+ 38	+ 34	− 53	+ 18	− 92
+ 51	+ 14	−51	− 26	+ 11	+10		+56			− 18	− 7	+ 7	+ 8	+ 2	0	+20	+ 4	+ 55	− 68	+ 25	+ 53	− 69	+ 28
+ 51	− 68	+87	+ 6	− 18	+13					− 18		− 2	− 7	− 2	− 7	+ 8	− 2	+ 11	− 45	− 11		+ 31	+ 46
− 6	+ 45	+68	+ 1										− 7	0	− 2	− 5	0	+ 20	+ 2	− 49		+ 20	+ 21
− 21		−32	− 14													− 5	− 7	− 1	+ 5				
+ 16		−85	− 53													− 7		+ 5	+ 2				
+ 2		+ 8	+ 41													− 7		+ 8	− 7				
− 1			+ 81															+ 8	− 12				
+ 43			+ 93															+ 8	+ 2				
+ 32			− 55															− 10	+ 29				
− 68			− 58															− 28	− 7				
+ 10																		+ 26	+ 20				
− 95																		+ 8	+ 19				
− 1																		− 1	− 25				
																		− 10	− 25				
																		− 24	− 16				
																		− 24	+ 49				
																		+ 8	+ 29				
																		+ 8					
																		+ 8					
																		− 10					
																		− 10					
																		+ 26					
																		+ 55					
																		+ 8					
																		− 50					

Class II.—*Observations during smaller Disturbances.*

+ 22	− 12	+87	− 51	+101	+71					0	+20	+42	−44	+13	− 9		+16	+ 8	− 34	− 72	− 67	+ 12	− 89
+ 10	+ 31	+52	− 1							+27	+65	−94		+13	+ 6			− 50	+ 86	− 60	− 45	− 7	−116
− 86	− 3	+96	− 7								+34	−36						*+ 66	− 68	+ 7	+ 17	− 72	− 94
+ 32	+ 19		− 75								+65	− 2						+ 8	− 62	− 38		+ 7	− 32
−138	− 39		+ 3															+ 55	− 98	− 86			− 53
− 15	− 39																	+ 8	− 76				
− 68	− 69																	+ 17	+ 16				
−104	+ 44																		+ 49				
− 45	− 88																		+ 20				
																			+ 72				
																			+ 11				
																			+ 38				

Class III.—*Observations during greater and more Abrupt Disturbances.*

−146	−123		+117	+125	−71					†−18								+151	+ 2	−102	−102	−150	− 18
− 68			+ 93	− 18															−141	+ 34	−102	−100	
−119			+ 1																	+ 68			
			+ 93																				

* The declination needle was on this occasion considerably to the west of its normal position.
† The declination needle was on this occasion considerably to the east of its normal position.

A cursory glance at Tables VII., VIII., IX. is sufficient to show us that the values of the departures for Class 2 are greater than those for Class 1 ; and that in like manner those for Class 3 are greater than those for Class 2. But since it may be objected that the means in Tables IV., V., VI., are not in all cases thoroughly determined, I may likewise remark, that the range of the departures follows the same rule as their absolute values. Indeed, the great characteristic feature of the disturbing force is the large range which it causes between the different values of earth-currents observed on the same line, and during the same hour of the day, while at the same time the departures are perhaps as often to the one side as to the other of the normal values, as far as this may be gleaned from a somewhat limited number of observations.

That this peculiar action of disturbances on earth-currents does not depend upon the absolute amount of the disturbing force, magnetically measured, will, I think, be rendered probable from the following considerations :—

1*st*, In the two cases in which the declination needle was considerably deflected from its normal position, there is no evidence from the corresponding earth-currents to show that these depend upon the absolute amount of the magnetic deflection.

2*d*, For all the other disturbed observations of earth-currents, neither magnet was far from its normal position.

While, however, there is no evidence in favour of the theory which ascribes magnetic disturbances to the direct action of earth-currents, there is much to favour the induction hypothesis in the great range towards either side of their normals apparent in the values of those earth-currents which correspond in point of time with magnetic disturbances. For it is evident that such a range from side to side would be produced if earth-currents were induced currents, due to those small but rapid changes in the magnetism of the earth, which are represented by the peaks and hollows of the disturbance curves.

Another remarkable feature of Tables VII., VIII., IX., is the tendency of the undisturbed observations in juxta-position with one another, to exhibit departures affected with the same sign ; and if we reflect that the order given in these tables is that of time, we shall, I think, be disposed to conclude with Mr WALKER, that meteorological conditions in all probability influence the values of the currents observed.

If this be the case, there would thus appear to be three independent phenomena which affect these values :—

1*st*, The daily magnetic change.

2*d*, Magnetic disturbances.

3*d*, Meteorological conditions, probably those which alter the conducting power of the superficial strata of the earth's surface.

If we reflect on this, and bear in mind that we have as yet obtained only a

limited number of observations of earth-currents, and that only at one place, it is surely too soon to endeavour to trace quantitatively the connection between such currents and magnetic phenomena. I have therefore confined myself to a kind of qualitative analysis, which perhaps may help roughly to determine the nature of this connection.

I shall be excused if I here call attention to some remarks on the subject of this paper made by the Rev. Dr LLOYD of Dublin, with the view of showing that the facts noticed by this eminent physicist may be explained on the induction hypothesis. These remarks occur in a paper "On Earth-Currents and their Connection with the Diurnal Changes of the Horizontal Magnetic Needle," which is published in the Transactions of the Royal Irish Academy.* The author says,—

"When we examine the curves, in which Mr BARLOW has represented the course of the galvanometric deflections caused by the earth-currents, we observe that the regularity of that course is continually interrupted by rapid reciprocating movements, in which the needle oscillates from one side to the other of the zero alternately. These movements are similar to those of the magnetometers with which we are familiar; but they are much more *rapid*, and bear a *larger* proportion to the *regular* changes. . . . . The *frequency* and *magnitude* of the deflections may both be taken into account, by adding together the alternate changes, without regard to sign, and dividing the sum by the regular daily changes. I have selected for this calculation the observations made during the six hours, commencing at 3 A.M., on May 29, 1848, that being a period of comparative disturbance. The sum of the changes of the galvanometer needle during that period, on the Derby and Rugby line, was equivalent to 571 divisions of the instrument,—the mean daily range for the entire week being 11·4 divisions, and the ratio = 50. The corresponding ratio for the galvanometer of the Derby and Birmingham line is somewhat smaller. The sum of the changes of the Greenwich declinometer during the same period was only 57 minutes, the mean daily range being 12·4 minutes. In like manner, the sum of the changes of the horizontal force (in parts of the whole) was ·0158, the mean daily range being ·0034. The *ratio* is accordingly the same for the two magnetic elements, and its amount is 4·6, or less than one-tenth of the corresponding ratio in the case of the galvanometric changes. We learn, therefore, that the rapid changes of the earth-currents are much greater in proportion to the regular daily changes, than the corresponding movements of the magnetometers."

The fact noticed by Dr LLOYD in these remarks is quite in accordance with the induction hypothesis, and is indeed only another way of expressing the truth already stated, that the action of disturbances on earth currents is to increase the

* Vol. xxiv. p. 115.

range of the latter, and that this does not depend on the absolute amount of the disturbing force, magnetically measured.

A single glance at the Kew disturbance curves will show any one adopting the induction hypothesis why the range of the earth-currents observed during disturbances is so very great. The chief characteristic of a disturbing force will then be seen to consist rather in *abruptness* than in *absolute amount* of change; and it may safely be affirmed that the abruptness of a disturbance exceeds that of the ordinary daily variation to a very much greater extent than the absolute amount of deviation from the normal caused by a disturbance exceeds the ordinary daily range.

But, on the induction hypothesis, the values of the earth-currents observed during disturbances will depend upon the abruptness of the latter—change in one direction producing positive, and that in the opposite direction negative currents; hence the range of such currents will be very great.

Dr LLOYD again remarks,—"The *calculated* curve is for the most part *above the observed*, especially in the Derby and Birmingham line. . . . . It may probably be accounted for by the fact, that the *zero* from which the magnetic deflections are measured is not the true one, corresponding to the absence of deflecting force. As we have no means of determining the latter, we are accustomed to take the *mean position* for the entire day, or the mean of the readings taken at equal intervals, as the point from which the deflections are measured. But there is reason to believe that this is not the true position of rest, corresponding to the absence of all disturbing force. The comparative quiescence of the magnets during the early hours of the morning seems to indicate that they are *then* near their true positions of equilibrium; and this indication is confirmed by the galvanometric curves, the zero line, which corresponds to the absence of current, dividing the area of the diurnal curve unequally, and being nearer to the night observations than to those of the day."

The following is the explanation of this fact afforded by the induction hypothesis:—"During the early hours of the morning, when the magnets are comparatively quiescent, and there is hardly any magnetic change, there will consequently be hardly any induced current, and hence the observations during these hours will approach very near to the zero of current.

The results of this investigation may be briefly stated as follows:—

1*st*, The earth-currents observed during periods of magnetic calm follow a well-marked daily law, one feature of which is the small value of those currents collected during the early morning hours; and this admits of being readily explained on the induction hypothesis.

2*d*, These observations are probably influenced by such meteorological conditions as affect the electrical conductibility of the upper strata of the earth's crust.

3*d*, The values of the earth-currents collected during periods of magnetic disturbance are chiefly remarkable for their great range, with frequent change of sign. This also admits of a simple explanation on the induction hypothesis.

4*th*, This hypothesis would therefore appear to be sufficient to account for all the earth-currents hitherto observed.

XXVIII.—*On the great Refracting Telescope at Elchies, in Morayshire, and its Powers in Sidereal Observation.* By Professor C. Piazzi Smyth.

(Read December 1862 and March 1863.)

PART A.—Instrumental Details, ... Pages 371-380
PART B.—Observational Particulars, ... 380–413
PART C.—General Deductions, ... 413-418

Part A.—1. *Introduction to Instrumental Details.*

The following pages contain an account of a few double-star measures which, by the kind permission of J. W. Grant, Esq., of Elchies, in Morayshire, I was enabled to make there in September 1862, with his large and equatorially mounted refracting telescope; and as that instrument is altogether the best and most powerful of its kind which has hitherto been erected in Scotland, such a trial of its capabilities, and the first which has been published, will undoubtedly have a peculiar interest for the Royal Society of Edinburgh.

The object-glass of the telescope is 11 inches in diameter, or rather its clear aperture, for the glass discs themselves may be a little more; while the next largest in Scotland, that recently acquired by the Glasgow Observatory, under its present able director, Professor Grant, is not more than 9 inches; and the chief object-glass of the Royal Observatory of Edinburgh, or that of the Meridian Transit Instrument, only $6\frac{1}{4}$ inches aperture. The light, therefore, of the Elchies telescope is, comparatively,* transcendent; and to enable this feature to be employed with the best effect upon his favourite stellar pursuits of earlier years in India, where extreme accuracy of measurement was always one of his chief desiderata, Mr Grant spared no expense in securing for the Elchies instrument an unusually efficient and well made equatorial mounting, fully provided with clockwork motion and micrometrical apparatus.

The order for the construction of this instrument would appear to have been given about the year 1849, a period when Mr Grant, though still in India, was just about to bring to a close his long service of forty-four years of continued official residence there: and it seems to have been commenced immediately;

* It would not be right to ignore that both England and Ireland have object-glasses of 12 inches in diameter; that the Russian Observatory of Pulkova has possessed for a quarter of a century, and used with great profit to exact astronomy, an object-glass of 15 inches in diameter; and that, in addition to Paris, the astronomers of the United States have now in employment one of the same size, and also one even of 18 inches in diameter, the normal size for a new generation, with similar advantageous results.

for, in 1851, the equatorial mounting, in its chief parts, was exhibited by Mr ANDREW ROSS, the optician employed, as his so-called "trophy" in the eastern half of the Nave of the Great Exhibition building of that year, with the effect of gaining for him one of the highest medals awarded. But after that event, lamentable delays took place in finishing the instrument; and before they were all concluded, Mr GRANT's health, too long tried by an Indian climate, unhappily broke down altogether. Hence it eventually occurred, that this fine instrument,—with the assistance of which, as the character of the work he had already executed in India with a smaller telescope sufficiently demonstrates, its owner would soon have risen to the first rank of acknowledged double-star observers in this country,—remained nearly unused until my going to Elchies in September 1862.

I had only intended on that occasion to pay a passing visit, as one of respect to the founder of the largest astronomical equatorial in Scotland; but Mr GRANT's hospitable notions, and his ideas of the importance of anything at all promising to be useful in practical astronomy, prevented my quitting his mansion until three weeks of observation had been secured, or something more than a mere amateur idea of what the telescope was capable of doing; and as he gave up the Observatory to me for the time entirely, and as I worked these alone, I can answer, and indeed am answerable, for whatever was done in it during that period, especially for any errors or shortcomings of my own.

A.—2. *Present Condition.*

The patrimonial estate of Elchies lies on the banks of the Spey, about 8 miles below the junction of that river and the Avon, in Lat. N. 57° 28′, and Long. W. 3° 15′ nearly. The house, which is on a considerably elevated plateau, stands, together with the observatory a few yards from its south-eastern corner, on a broad lawn surrounded by well-grown trees. There is an ornamental portico to the observatory, decorated, not at all inappropriately, with Egyptian emblems, carved in native stone, and also a small transit room; but the chief bulk of the whole structure consists of the large circular equatorial room, about 25 feet in diameter, and furnished with a metal-covered conical dome.

This dome has attached to it wheels of one foot in diameter; and, for motion, is revolved on them, while they roll on a fixed circular rail attached to the wall; impulse being communicated by means of toothed gearing and a hand-crank, which acts sufficiently easily. The shutters of the dome, four in number and arranged in two pairs, an upper and a lower one, open to the sky right and left. This they do by sliding on and off the opening of the dome in a peculiar, and as far as I know, a novel manner, with a sort of parallel motion movement in their own plane; each shutter being carried by two pivots, formed in the ends of two strong iron arms, which again work on their own fixed centres on the dome, and

have counterpoise weights beyond. These shutters were always most satisfactory and true in their movement, opening and closing with facility, and either offering a very large and broad view of the heavens, or keeping out both wind and wet with perfection.

The equatorial *mounting* is of that character usually known as the German variety in form, though in this instance it is constructed in the stronger manner of English engineering work; and has been so abundantly described already in print—1st, in the Jury reports of the Exhibition of 1851; and, 2d, in the Royal Astronomical Society's Monthly Notices for November 1862—that little more need be said of that part of the subject here. Though indeed it is proper to record, considering the great size and weight of the instrument, that I found the handling and working of it by myself alone, far more easy than I had expected; experiencing too, throughout the whole of the observations, a remarkable freedom from tremors either of tube or mounting; a consequence, without doubt, of the superior weight, and very massive construction of all those larger parts, which the optician, Mr Ross, had wisely confided to the hands of the well-known manufacturing firm of RANSOMES and MAY, at Ipswich, to execute for him.

A.—3. *Preparations for Observing.*

These preparations consisted in little more than cleaning the instrument from old and hardened oil, cleaning both outer and inner sides of object-glass, but without separating the lenses; removing paint from, and brightly polishing, the outer surface of the metal dew-cap; *reddening* the lamp illumination of the field of view, determining the magnifying power of the eyepiece employed throughout (and found to be 397),* testing the equatorial adjustments and micrometer values, chiefly by daylight observations on known stars, and then in preparing the list of objects to be examined. These were, for the most part, selected from the double and compound stars which I had begun to observe on the Peak of Teneriffe in 1856, but had had no opportunity of reobserving since then.

It was part of the preparation also to endeavour to form some idea of the quality of the object-glass about to be employed; an object-glass which, though furnished by Mr Ross, is said to have been actually constructed in the optical factories of Munich. There are many small bubbles in the material, but no perceptible striæ; and the discs which are given to the stars when in focus are extremely small,—so small that the two stars B and C of γ Andromedæ, stars of the fifth and sixth magnitudes respectively, and 0·6 of a second apart, were on one occa-

* This high power could be kept constantly on, without inconvenience when first picking up any small star, owing to the luxurious furnishing of finders to the large telescope, for it had no less than three such appendages; whereof the first had a 4·5-inch object-glass, and a magnifying power of 56 times; the second, a 2·2-inch object-glass, and magnifying power of 28 times; and the third, a 1·7-inch object-glass, and magnifying power of 17 times.

sion seen completely separated, well-formed, and with dark sky between them; a decided advance on what I have ever witnessed with 7 and 9-inch object-glasses elsewhere by celebrated makers.

The larger luminous discs also, which the Elchies object-glass gives to stars, on being thrown much out of focus,—and *a* Lyræ was the one principally experimented on,—were pretty regular in their circular formation and strength of illumination, though they had a disagreeable feature of colour; for when the eye-piece was pulled *outwards* from the focus, the disc was greenish, with a violet border or fringe; but when pushed *inwards* therefrom, there appeared a small central disc of violet, with an annular surrounding space of green; and when set *exactly* to the focus, the central part of the star was white, or slightly yellowish, astonishingly brilliant, and surrounded with rays that showed an intense violet colour towards their outer ends.

These effects, as I presume, are chiefly those of the "residual spectrum" of the object-glass, a defect that must exist to some extent in every so-called achromatic, but varying in character in each case with the judgment of the optician and the nature of the glass at his disposal. In the Elchies object-glass, as the outstanding defect seemed chiefly centered in the violet ray, which contains but a very small portion of light, its consequences may not be of moment in most of the ordinary optical observations, though it may become of importance in any attempt to use the telescope photographically, and did seem to prejudice the eye-estimations of the colours of some few stars. For instance, when *a* Lyræ entered the field, the eye was so excited by the splendour of the great star, a real first magnitude, and by the glory of its violet halo, that the small companion star, which is only of the eleventh magnitude, and has appeared to me in other telescopes of a blue colour, assumed in this one a reddish look, apparently from the complementary effect of the dominant violet of the larger. This case, however, is an extreme one, and when the stars under observation were below the third magnitude, little, if any, of the violet halo was ordinarily noticed.

A.—4. *Identification of Objects Observed.*

For the large stars, so few in number, their mere names, as every one knows, are quite enough to identify them; but for the constantly increasing hosts of the small stars that are dealt with by modern astronomers, no system of names has ever proved sufficient. In such case, reference has first been tried to their *numbers*, as they stand in some numerically arranged catalogue; and if every observer was to refer, or could refer, to one and the same catalogue, that might form a practicable method. But this is not done, and for many various reasons; recourse being had instead to the numbers in a *variety* of catalogues, one man making one catalogue his standard of reference, and another, another.

Hence considerable confusion has crept in, where a mere number-reference is employed, and great loss of time is experienced in tracing the measures of any one star, through the catalogues of different observers arranged and indexed merely according to their own separate independent and peculiar numbers, which numbers too, in no case indicate the precise point of the sky to which a telescope should be directed in order to find the star. Some natural method therefore should evidently be made to over-ride all these artificial systems; and none seems better than a statement of the Right Ascension and Declination at a given epoch: I have therefore been much more particular in designating each of the stars in the Elchies list by that method, than by reference to any name or catalogue number. One of these is, indeed, usually given as an approximate step to begin with, but is always followed by the *place* for 1862, as brought up from the "Cycle," the chief book of reference employed.

Identification, however, thus by place and epoch, has its limits when stars are very close together; especially as the pointing of an equatorial may often be in error, from an accumulation of causes, to the extent of 1, 2, or 3 minutes of space. In such case, recourse must evidently be had to such further natural features as may be presented by the objects in the field of view; and which features do indeed form the chief portion of an equatorial's work to ascertain and record, viz.,—

Magnitude, or brightness.
Colour.
Relative position angle, when more than one star is concerned; and
Distance, under the same circumstances.

This consideration is evidently not new, though it has not been by any means so generally employed as it might have been with advantage to science; for with stars, at first thought nearly similar, most manifold physical peculiarities have been found on closer study. I have endeavoured, therefore, to take more account than has usually been the case, of all the prominent characteristics of every star observed; and have not only arranged all my own results in that manner, but those of other observers referred to for comparison, as well; setting down each person's work in order of date, to the end that any large cosmical progressive change in any of the elements may be instantly manifested.

When the cosmical change, however, is small, or the previous authorities scanty, and the Elchies observation simply shows a *difference* from what had been expected, then comes the all-important question, as to within what limits can we depend on an Elchies observation; for on the answer given thereto will it rest, whether the difference noted may most probably be put down to mere error of observation; or, quite certainly assumed as showing a cosmical change in the objects observed. A formal inquiry into these conditions will be all the

more proper in the present case, and respectful from myself to older observers, seeing that double-star measuring is one of the most refined and difficult branches of practical astronomy, where few persons succeed without much longer experience of it than has fallen to my share; and in which, from the nicety of the micrometrical apparatus employed, it is always possible to read off an observation, and record it on paper to very many more decimal places than it can really be trusted to.

A.—5. *Probable Error of Observation.*

Now these errors in my work at Elchies I believed were always large, not only for the reasons given above, but also because the number of nights that any object was observed was seldom more than two or three, and sometimes only one; and because, too, the definition of the atmosphere was almost invariably, on every occasion that I looked through the telescope, so extraordinarily bad, that every star, instead of appearing like a proper typical stellar point, was blurred into a more or less, and often exceedingly large, and therefore correspondingly faint, nebulous patch or wisp of light.—(See Royal Astronomical Society's Monthly Notices for November 1862, p. 3, and p. 13.)

Whereas, therefore, most observers assign to their best measures a weight of 10, and to their worst a weight of 1 only; I, while keeping to their upper limit for an ideal best, have often in practice found it expedient to go lower still than their lowest, and to divide their 1 into tenths.

These numbers will accordingly be found set down against every Elchies observed quantity; and it is expected that any astronomer who hereafter may use the quantities for purposes of his own, will duly refer to the numerical weight attributed by their observer. Yet the numbers, after all, indicate only an opinion or marked impression, which may be biased either way; and the number of observations of each object is too small to allow of estimating the degree of probable error on any rigid mathematical basis, with a logical prospect of practical success; something else therefore, and more suitable to the case, is absolutely required; and with the view of attempting to furnish such a desideratum, I have compared my Elchies observations, in each of the four features above enumerated, with the mean determinations brought up from the "Cycle," and other standard authorities; *i.e.*, whenever the stars concerned appeared to be relatively fixed and constant, or very nearly so, in order to avoid being misled by any large change, depending on time.

The results of this comparison are given in the two tables below, whereof the first refers to "Magnitude" and "Colour," which are mere matters of approximate estimation; and the second to "Position" and "Distance," which, being affairs of micrometric measurement with all available accuracy, have had more care bestowed upon them.

CORRECTION—TABLE I.

Star Group.	Component.	Magnitude.		Colour.	
		Standard authority.	Elchies observed.	Standard authority.	Elchies observed.
35 Piscium	A	6	6	... ...	... ...
	B	8	8	... ...	... ...
65 Piscium	A	6	6·3	Pale yellow (?)	Bluish white
	B	7	7·0	Pale yellow	Faint yellowish white
Ψ Piscium,	A	5·5	5·0	Silvery white	Yellowish white
	B	5·5	5·5	Silvery white	Bluish white
γ Arietis,	A	4·5	4·8	Bright white	Greyish white
	B	5·0	4·9	Pale grey	Slightly reddish white
λ Arietis,	A	5·5	5·3	Yellowish white	Yellowish white
	B	8·0	8·8	Blue	Light blue
α Herculis,	A	3·5 (?)	3	Orange	Coppery orange
	B	5·5 (?)	5	Emerald	Greenish
70 Ophiuchi,	A	4·5	5	Topaz yellow	Yellow
	B	7·0	7	Variable	... ...
β Cygni,	A	3·0	3·0	Topaz yellow	Yellow
	B	7·0	7·0	Sapphire blue	Bluish green
γ Delphini,	A	4	4·5	Yellow	Pale yellow
	B	7	7·0	Light emerald	Slightly greenish
1 Pegasi,	A	4	4·5	Pale orange	Yellowish
	B	9	9·0	Purplish	Grey

CORRECTION—TABLE II. (*See Note to p.* 415.)

Star Group.	Approx. Position.	Approx. Distance.	Position.			Distance.		
			Corrections for Elchies Result.	No. of Nights Obs.	Supposed Weight.	Corrections for Elchies Result.	No. of Nights of Obs.	Supposed Weight.
35 Piscium,	149°	12″	−0° 7′	2	2	+0″·03	2	1
Ψ Piscium,	160	30	0 0	2	2	−0·28	2	1
ζ Piscium,	63	23	+0 9	2	2	−0·20	2	1
γ Arietis,	0	9	−0 56	3	3	+0·20	3	2
γ Andromedæ,	62	11	+0 3	2	2	+0·69	1	0·3
32 Eridani,	347	7	−0 10	1	1	+0·08	1	0·5
α Herculis,	119	5	−0 10	3	2	+0·07	2	0·8
ϱ Herculis,	310	4	0 0	2	1	−0·05	2	0·8
95 Herculis,	261	6	−0 20	2	1	−0·19	2	0·6
β Cygni,	55	35	+0 3	2	2	−0·10	2	1
ζ Sagittæ,	313	9	−0 40	3	2	−0·16	3	1
γ Delphini,	273	12	+0 30	2	1	+0·25	2	0·5
Mean, according to signs,		14″	−0° 8′			+0″·03		
Mean disregarding the signs,		14	0 16			0·19		

A simple inspection of the first of the two previous tables may be considered to show, that in *magnitude* or brightness, an Elchies observation is not likely to be in error more than 1 magnitude on the "Cycle," which follows very nearly the Herschelian scale; and in *colour*, though the estimation was rude, and based entirely on my own independent ideas of colours, yet the Elchies difference from the same standard authority is always comprised within some small variety of one and the same tint, indicating, therefore, the non-existence of chromatic error or equation, personal, instrumental, or geographical, to any large or sensible amount.

Similarly the second table may be held as exhibiting, that there are hardly any errors of a constant order worth noticing, on either position angles or distances,* as observed at Elchies, while the occasional errors, + and −, are confined within a degree for "positions," and a second for "distances," over cases varying in absolute distance from 4″ to 35″.

These limits of occasional possible error, though they include some cosmical uncertainties in the progress of the stars themselves, may appear rather large when compared with the many decimal places adopted by some few single observers in recording their observations; but will appear more moderate when we come presently to compare the results of many such first-rate observers *inter se*; and will even in the end be found very small, compared to the important natural changes which have undoubtedly been undergone by some of the stars since the last previous observations that have come to my knowledge.

A.—6. *Stars observed.*

The following table contains a condensed list of the stars or star-groups observed at Elchies, and arranged in order of Right Ascension:—

Name.	Approx. R. Ascension for 1862, Jan. 1.	Approximate Declination for 1862, Jan. 1.	Name.	Approx. R. Ascension for 1862, Jan. 1.	Approximate Declination for 1862, Jan. 1.
	h. m. s.	° ′ ″		h. m. s.	° ′ ″
35 Piscium, .	0 7 52	+ 0 3 14	η Serpentis, .	18 14 11	− 2 55 35
65 Piscium, .	0 42 28	+26 57 31	α Lyræ, . .	18 32 14	+38 39 8
ψ Piscium, .	0 58 17	+20 44 1	28 Aquilæ, . .	19 13 14	+12 7 22
ζ Piscium, .	1 6 37	+ 6 50 44	δ Aquilæ, . .	19 18 31	+ 2 50 25
γ Arietis, . .	1 45 58	+18 37 6	128 Anseris, . .	19 20 26	+19 37 12
λ Arietis, . .	1 50 15	+22 55 20	β Cygni, . .	19 25 9	+27 40 20
222 Arietis, . .	1 51 56	+20 23 14	ζ Sagittæ, . .	19 42 52	+18 47 57
α Piscium, .	1 54 54	+ 2 5 45	α Aquilæ, . .	19 44 2	+ 8 30 5
γ Andromedæ,	1 55 25	+41 40 3	γ Delphini, .	20 40 17	+15 37 54
32 Eridani, .	3 47 24	− 3 21 52	452 Cygni, . .	20 57 4	+38 58 0
α Herculis, .	17 8 21	+14 32 51	1 Pegasi, . .	21 15 43	+19 12 56
ρ Herculis, .	17 18 56	+37 16 35	3 Pegasi, . .	21 30 51	+ 6 0 2
95 Herculis, .	17 55 38	+21 35 55	312 Pegasi, . .	21 45 6	+19 10 53
70 Ophiuchi, .	17 58 28	+ 2 32 32	Σ 2841, . . .	21 47 46	+19 2 53

* The reading of the equator on the position circle of the Elchies micrometer was obtained every observing night, in the usual manner, by equatorial stars being made to "thread" along a

A, 7.—*Proper Motion Effects.*

Though the list of stars observed at Elchies was small, yet it contained not a few instances where previous observers were more or less uncertain, whether the group should be classed as a physical and real, or only as an optical and unreal or apparent, double star. Some of these cases have been affirmatively settled by the Elchies micrometrical observations exhibiting proofs of orbital movement, and others by the test of "proper motion." This latter, however, being applied in rather an extreme manner, I propose to introduce at this point a tabular view of the data, and trust that it will be found a convenient and important reference throughout the next section of the paper.

The "proper motions" employed, are those of the British Association Catalogue, where they are given in their annual amounts, generally for the larger component only of each star group. If in any particular instance, the smaller members of the group form a physical system with the larger, evidently they will experience the same amount of proper motion,—whether real from their own movement, or apparent from our sun's,—and their relative "positions and distances" will not be altered thereby. But very different results will follow, if the members of any star-group are not physically connected together, and only the larger component be endued with the British Association Catalogue recorded amount of Proper Motion; and if the accumulated effects of such motion can be traced through a considerable period of time. Now, the greater part of the "Cycle" references being about thirty years earlier than the Elchies work, I have multiplied the British Association proper motions by that number, and then computed their effect in altering the relative positions and distances of the members of each star-group concerned, on the hypothesis of their being in apparent proximity to each other only.

The result is usually an alteration so greatly exceeding the bounds of probable error in an Elchies observation, that, according as it, the alteration in position and distance, is or is not borne out and reproduced by, and in, these observations, we are enabled to state with great confidence (subject only to the truth of the said British Association Catalogue proper motions), whether the group belongs to the one or other leading variety of double stars.

wire. The value of the micrometer screw in seconds of space was derived from transits of the pole-star, measured with a sidereal pocket chronometer, kindly lent by Dr Lee of Hartwell to the Edinburgh Royal Observatory for many years, and brought northwards on this occasion.

Star or Star-Group.	Annual B. A. C. Proper Motion.		Relative State rudely			
			Observed in 1832.		Computed for 1862.	
	R. A.	N. P. D.	Position.	Distance	Position.	Distance.
35 Piscium, . . .	+0″·10	+0″·04	150°	11″·5	162°	9″·2
65 Piscium, . . .	+0·08	−0·03	299	4·5	282	6·2
ψ Piscium, . .	+0·15	+0·02	160	30·0	162	30·0
	+0·10	+0·03				
ζ Piscium, . .	+0·20	+0·06	63	23·5	60	23·5
	+0·28	+0·02				
λ Arietis, . . .	−0·08	0·00	46	37·0	48	38·5
γ Andromedæ, . .	+0·06	+0·04	62	10·0	52	9·5
32 Eridani, . . .	+0·10	0·00	347	6·5	325	7·8
α Herculis, . . .	+0·03	−0·05	119	4·7	140	5·0
ε Herculis, . . .	+0·06	−0·02	310	3·6	292	4·5
95 Herculis, . . .	+0·02	−0·06	262	6·0	248	7·0
η Serpentis, . . .	−0·56	+0·65	71	133·0	66	155·5
α Lyræ, ...	+0·30	−0·28	139	43·2	153	46·5
δ Aquilæ, . . .	+0·27	−0·11	260	96·0	259	104·5
128 Anseris, . . .	−0·03	+0·06	43	22·7	42	24·6
β Cygni, ...	+0·03	−0·02	55	35·0	55	34·0
ζ Sagittæ, . . .	+0·10	−0·07	313	8·6	292	9·8
α Aquilæ, . . .	+0·51	−0·38	320	155·0	313	157·0
γ Delphini, . .	0·00	+0·15	273	11·8	280	12·5
	+0·02	+0·20				
1 Pegasi, . . .	+0·18	−0·09	311	37·0	303	39·6

Part B.—*Observational Particulars.*

The Stars, as observed and compared, arranged in order of Right Ascension.

(1.) 35 PISCIUM, Approx. R.A. $0^h\ 7^m\ 52^s$; Decl. +8° 3′ 14″ for January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 148° 54′	12″·50	1783·50	H[1].
A	...	.....				
B	...	.....	AB 151 6	11·28	1821·93	σ.
A	...	.....				
B	...	.....	AB 150 46	11·17	1821·91	H[2] and S.
A	...	.....				
B	...	.....	AB 150 6(*w*8)	11·60(*w*7)	1832·04	Cycle.
A	6·2	White				
B	7·8	White	AB 149 52	11·53	1832·67	Σ.
A	6·5	.....				
B	8·0	.....	AB 149 21	11·66	1836·74	Σ.

PART B.—*Observational Particulars.* 35 PISCIUM—*continued.*

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	6	Pale white				
B	8	Violet tint	AB 149° 30′(*w*7)	11·90″(*w*7)	1837·89	Cycle.
A	...	Yellowish				
B	...	Azure	.....	.....	1844·8	Sestini.
A	...	White				
B	...	Purplish	.....	.....	1850·7	Spec. Hart.
A	...	Yellowish(2)				
B	...	Pale violet(2).....	.....	.....	1856·68	Alta Vista.
A	6 (3)	Yellowish white(3)				
B	8 (3)	Greyish(3)	AB 149 49* (*w*2)	11·64(*w*1)	1862·72	Elchies.

Mean of four best results, 1832–1838; position AB 149° 42′; distance 11″·60; date 1834·84.
Elchies corrected for constant differences; position AB 149° 41′; distance 11″·67; date 1862·72.

When the results of different observers are arranged in this clear and tabular manner, it seems, at first sight, so extremely easy to judge by mere inspection, whether our knowledge of the star is in a satisfactory condition or otherwise, that it is needless to add anything to them. Yet a very little further examination will soon reveal, that there are a great many unknown quantities to be taken into account, and that it is often extremely difficult to come to a decision between what shall be attributed to error of observation in a former observer, and what to a real change in the star or star-group itself; and, according as the decision may be given one way or the other, so the new Elchies observation may fulfil a more or less useful part in the history of the star. At all events, it is only by such a comparison, rigidly, impartially, and extensively carried out, that we can really arrive at any definite idea, as to whether the Elchies observations are in themselves good or bad, (merely to have printed them by themselves, would have proved nothing), and whether they have haply added anything to

* The numbers in simple parenthesis after an Elchies observation, indicate the number of nights on which it was observed in any particular element; but when a *w* is prefixed to the number, it indicates the estimated weight concluded on the principles mentioned in page 376. The original authorities referred to for numerical observations, are,—

"Mensuræ Micrometricæ" of F. G. W. von Struve, quoted as Σ, when observing with the 9-inch, and σ, with the 3·6-inch Dorpat Equatorial.

"Cape of Good Hope Observations," by Sir J. F. W. Herschel, quoted as H^2.

"Cycle," by Admiral W. H. Smyth, 1844.

"Speculum Hartwellianum," by the same, 1860.

Memoirs and Monthly Proceedings of Royal Astronomical Society; Astronomisches, Nachrichten, by Dr A. Auvers; and Edinburgh Astronomical Observations, vol. xii., for Guajara and Alta Vista observations in Teneriffe. Other references to Sir William Herschel, as H^1, and Sir John Herschel and Sir James South, as H^2 and S, are extracted either from the "Mensuræ Micrometricæ," or the "Cycle"

our exact knowledge of sidereal phenomena. It may therefore be of some preliminary assistance, if I call attention to a few of the more noticeable features which have struck me, after having thus brought together the several authorities.

Beginning then with the columns on the left-hand side of the page, it may be remarked, that—

In Magnitude, these stars show no very sensible alterations within the period over which all the observations extend.

In Colour, a slight gradual change in both components may be suspected.

In Position and *Distance*, there is something to discuss. Looking up and down these columns, the reader may first be struck by the greater differences,—and, evidently by their + and − character, greater errors of observation,—among the earlier than the later observers. This is much as might be expected in the nature of things, and is not mentioned invidiously, but only as a point upon which a practical judgment must be formed, and a case where we should try to avoid being led away impulsively by the splendour attached to great names. Had the two earliest sets of observations, by "H^1" and "σ," been the only ones in existence, a rapid change of the stars in both position and distance would have been implied, and believed in to this day. But subsequent observations soon impressed both "Σ" (the elder STRUVE at Dorpat), and my father in his "Cycle," that so far as *Position* was concerned, no sensible change was going on; and this view is remarkably borne out by the Elchies observation, at an interval of twenty-eight years after a mean of four of the best sets of preceding observers. Indeed, it differs therefrom by only 1′; a pure accident in an instance where the third part of a degree would have been excusable; and a coincidence certainly, which I had no idea of, until after the authorities had been collected, and that was very long after the observations were made.

In *Distance*, my father thought the stars constant, but STRUVE (pp. 137 and 281 of his great Catalogue), after having rejected the H^1 observation as erroneous, thinks that a slow increase is going on, especially when taking account of constant errors; which, being corrected for in earlier observations, would make his measure of $1821{\cdot}93 = 11''{\cdot}21$, and that of H^2 and S in $1821{\cdot}91 = 10''{\cdot}58$, leaving a large increase for his subsequent observations of 1832 and 1836. The Elchies observation however again shows, and by an almost marvellous agreement, no increase to have taken place in the last twenty-eight years.

May we trust this result? I think we may; but am not so much guided thereto by the Elchies result, to which the attributed "weight" is exceedingly small, as by some considerations on the older measures. All the world knows the elder STRUVE as a first-rate observer, and when he observes with the large Dorpat telescope of 9 inches aperture, no one can come near him. This class of his observations is marked with a Σ, and the record of 1832 is the mean of no less than seven sets, in five different years; something of this sort too being gene-

rally the case with a given Σ result, such a reference must be looked on with exceeding respect. But STRUVE observing with the later 9-inch Dorpat, is a very different person in his results from the same STRUVE working with the earlier 3·6-inch Dorpat, or the result given as "σ;" and it is chiefly on the $\sigma - \Sigma$, than the $\Sigma - \Sigma'$, that the argument for change in the star 35 Piscium used to be founded. The argument, though weak, was no doubt contributed to also by the H^2 and S observation, and in a more exceeding degree; but if I am right in my belief that the small size of STRUVE'S earlier telescope is the reason of the σ results being always greatly inferior in accuracy to those marked Σ,—then a similar cause should act and to a large extent in vitiating the excellence of an "H^2 and S" result, transcending though the natural observing powers of these eminent men may have been. In fact, too, we shall soon find, before we have gone very far through our list, that the limits of error of "H^2 and S" are decidedly large; and though those of the excellent "H^1" are often larger (by reason of the imperfect mounting, rather than the defective size, of the instruments he employed), yet in this case it would have been better to take a mean between them all, than to have altogether rejected H^1. If too such a mean be now taken, then all the rest of the results, including that of Elchies, confirm my father's view of the cosmical constancy of this star-group in distance as well as position.

But in *character*, what are they, these two stars; do they form a physical or an optical pair? Now extreme relative fixity, such as here prevails, has usually I believe been put down as implying that a pair is optical only; and our proper motion table, on page 380, will enable us at once to test that hypothesis on the foundation there adopted; for the large star has so sensible an amount of proper motion attributed to it, that in thirty years the angle of position should have increased by no less than 12 degrees, and the distance lessened by 2·3 seconds, if the larger is not carrying the smaller orb along with it in its flight. But as we have already seen, no such change has occurred, certainly nothing equal to the twentieth part of what is here indicated; we are therefore justified (subject always to the correction of the British Association Catalogue), in considering the pair to be a physical double star, or a binary with extremely slow orbital movement.

(2.) 65 PISCIUM, Approx. R.A. $0^h\ 42^m\ 28^s$; Decl. $+26°\ 57'\ 31''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 300° 57′	4″·0	1783·15	H_1
A	...	.....				
B	...	.....	AB 297 22	.....	1802·61	H_1.
A	...	.....				
B	...	.....	AB 295 48	.....	1822·86	H_2 and S.

65 PISCIUM—*continued.*

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	6	Pale yellow				
B	7	Pale yellow	AB 298° 12′ (*w* 6)	4″·2 (*w* 4)	1830·97	Cycle.
A	6·0	Yellowish				
B	6·2	Yellowish	AB 298 58	4·45	1832·13	Σ.
A	...	.....				
B	...	.....	AB 298 30 (*w* 8)	4·5 (*w* 5)	1838·17	Cycle.
A	...	Yellowish				
B	...	Azure	.. ...	.....	1844·8	Sestini.
A	...	Pale yellow				
B	...	Pale yellow	.....	.....	1850·8	Spec. Hart.
A	...	White (1)				
B	...	White (1)	.....	.....	1856·68	Alta Vista.
A	6·3	Bluish white (2).....				
B	7·0	Faint yellowish white (2)	AB 298 28 (*w* 2)	4·55 (*w* 1)	1862·73	Elchies.
		Mean of 3 best obs. 1830–39,	Pos. = 298°33′	Dist. = 4″·38	Date = 1833·72.	
		Elchies corrected,	Pos. = 298°20′	Dist. = 4″·58	Date = 1862·73.	

In *Magnitude*, these stars seem steady.

In *Colour*, they are weak, but may be slightly variable interchangeably.

In *Position* and *Distance*, they are a remarkable lesson; for whereas the first three sets of measures showed a change of 5° of position in forty years, on the strength of which an orbit was computed of one star round the other in 3000 years,—subsequent measures have proved that there was no sensible change at all, and the appearance of it was merely due to the large errors of observation of the earliest authorities, and to their chancing to tend in one direction. If further proof of the "fixity" were needed, it is supplied, as will be seen, by the Elchies observations.

In *Character* the stars may still be termed physical, or slow binaries, by the entire non-appearance of the B. A. C. proper motion, which, on the optical hypothesis, ought to have decreased the position by 17°, and increased the distance by 1·7″ in thirty years.

(3.) ψ PISCIUM, Approx. R.A. $0^h\ 58^m\ 17^s$; Decl. + 20° 44′ 1″, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	... ..	AB 170° 0′	27″·50	1779·83	H_1.
A	4·7	.....				
B	5·2	.....	AB 161 0	30·04	1821·93	σ.

ψ PISCIUM—*continued*.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 161° 2′	30″·34	1822·38	H^3 and S.
A	4·9	White.....				
B	5·0	White.....	AB 160 19	29·90	1832·11	Σ.
A	5·5	Silvery white				
B	5·5	Silvery white	AB 160 24 (*w* 8)	30·2 (*w* 8)	1833·97	Cycle.
A	...	Fine azure				
B	...	Fine azure	.....	...	1844·8	Sestini.
A	...	Flushed white.....				
B	...	Pale white	.....	...	1849·7	Spec. Hart.
A	...	White (1)				
B	...	White (1).....	.....	...	1856·68	Alta. Vista.
A	5·	(2) Yellowish white(1).....	.....	...		
B	5·5	(2) Bluish white (1).....	AB 160 22 (*w* 2)	30·43(*w* 1)	1862·72	Elchies.
C	12·5	(2) Bluish (1)	AC 121 20 (*w* 1)	98·18(*w* 1)		

In *Magnitude*, A and B are pretty constant.

In *Colour*, more attention to minute differences is required.

In *Position* and *Distance* very little change has taken place, and in a direction rather contrary to the computed effect of the *difference* of the proper motions in the B. A. C., where it is given to either of the components separately. This forms, however, only a "residual" anomaly, and it would be well worth while to have the full amount of the whole proper motions of the mean of the two stars, as given in the B. A. C., duly checked by micrometrical measurement. To this end the Elchies telescope showed a small star conveniently situated, not observed previously by any observer that I am aware of, and which I have called C, and measured as above.

In *character*, a slow binary.

(4.) ζ PISCIUM, R.A. $1^h\ 6^m\ 37^s$; Decl. $+6°\ 50'\ 44''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 67° 23′	22·19	1779·80	H_1
A	5	.....				
B	6	.....	AB 65 36	23·33	1823·87	σ
A	4·2	White				
B	5·3	White.....	AB 63 43	23·46	1832·83	Σ.
A	6	Silver white.....				
B	8	Pale grey	AB 63 48 (*w* 9)	23·4 (*w* 9)	1839·05	Cycle.

ζ PISCIUM—*continued*.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	Yellow	° ′	″		
B	...	Dingy yellow	.....	...	1844·8	Sestini.
A	...	.....				
B	...	.....	AB 63 18	23·1	1846·89	Spec. Hart.
A	...	White				
B	...	Greyish	.....	...	1849·7	Spec. Hart.
A	...	Yellowish (2)				
B	...	Warm grey (2)	.....	...	1856·68	Alta Vista.
A	...	.....				
B	...	.....	AB 62 54	22·8	1857·93	Spec. Hart.
A	4·0	.....	AB 64 2	...	1861·02	A. Auwers.
B	8·2	.....	.....	23·744	1861·74	
A	5·5 (1)	Yellowish white (3)				
B	7·5 (1)	Greenish grey (3)	AB 63 20 (w 2)	23·42 (w 1)	1862·72	Elchies.

In *Magnitude*, variable through rather more than one magnitude.

In *Colour*, also sensibly variable.

In *Position* and *Distance*, nearly stationary, much in accordance with the *difference* of the proper motions attributed to either star in the British Association Catalogue. These proper motions are absolutely very large, and their non-appearance on the relative places of the stars, indicates them to be,

In *Character*, slow binaries.

Since writing the above, I have become acquainted with the observations of Dr A. Auwers, taken with Bessel's celebrated heliometer, and now inserted in our tabular view; they are the mean of many observations extending over four years, and will command great weight. As such, it is singular to observe how they strengthen the character of the Elchies observations, of position and distance, making them appear nearly a mean between the best German and the best previous English measures of this nearly "fixed" star-group. In Dr Auwers' discussion of his own observations, he very properly compares them with those of previous observers, and gives the names of six to whom he has so referred. Unfortunately for impartial scientific knowledge freed from the bias of national forms of instruments and modes of observing, the whole of the learned Doctor's references are German, without exception. Yet, notwithstanding that that is the case, it is instructive to observe that the variations of the German results *inter se*, though depending on large numbers of observations, are not less than what was deduced as the broadest limits of possible error for the Elchies scanty observa-

tions, compared with other and various authorities, Dr AUWERS' table standing thus*:—

ζ PISCIUM.

Observations. Weight.	Position.	Distance.	Date.	Authority.
29·24	63° 55′	23″·364	1830·90	Bessel.
8	64 6	23·740	1834·82	Bessel.
48	63 50	23·499	1842·08	Schlüter.
8	64 43	24·200	1853·82	Peters.
20	63 16	23·145	1855·58	Luther.
33	64 2	.....	1861·02	Auwers.
20	.....	23·744	1861·74	

(5.) γ ARIETIS, R.A. 1h 45m 58s, and D. + 18° 37′ 6″, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 356° 5′	10″·17	1779·68	H[1].
A	...	.....				
B	...	.....	AB 356 54	8·97	1821·91	σ.
A	4·2	Very white				
B	4·4	Very white	AB 359 59	8·63	1830·84	Σ.
A	4·5	Bright white.....				
B	5	Pale grey	AB 359 48 (w 9)	8·8 (w 9)	1837·93	Cycle.
A	...	White .....}				
B	...	White .....}	.....	.....	1844·9	Sestini.
A	...	.....				
B	...	.....	AB 358 54 (w 8)	8·7 (w 6)	1849·12	Spec. Hart.
A	...	Full white.....}				
B	...	Faint blue.....}	.....	.....	1850·7	Spec. Hart.
A	...	.....				
B	...	.....	AB 358 19	8·87	1855·95	E. Luther.
A	...	Yellowish white (2) ...				
B	...	Yellowish white (2) ...			1856·68	Alta Vista.
A	4·1	.....	AB 359 15 (39)		1861·01	Auwers.
B	4·3	.....	AB	8·628 (22)	1861·85	
A	4·8(3)	Greyish white				
B	4·9(3)	Slightly reddish white(3)	AB 0 36 (w 3)	8·61 (w 2)	1862·73	Elchies.

One of the oldest known of telescopic double stars; in *Magnitude*, nearly constant.

* Astronomische Nachrichten, No. 1393, p. 10.

In *Colour*, some slight variations, chiefly in the smaller component.

In *Position* and *Distance*, nearly constant, but with a certain slow oscillation, apparently of a perturbative character, and with no differential proper motion; therefore

In *Character*, a slow binary.

(6.) λ ARIETIS, R.A. $1^h\ 50^m\ 15^s$; D. $+22°\ 55'\ 20''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 48 0	36″·61	1779·83	H[1].
A	5	.....				
B	7·7	.....	AB 44 0	($\Delta\delta + 25·91$)	1822·10	σ
A	5·5	Yellowish white				
B	8·0	Blue	AB 45 36 (w 7)	36·9 (w 7)	1830·96	Cycle.
A	...	White	.....	.....	1844·9	Sestini.
B	...	Pale azure.....				
A	...	Pale yellow (1).....	.....	.....	1856·68	Alta Vista.
B	...	Light lilac (1)				
A	....	Pale yellow	.....	.....	1857·7	Spec. Hart.
B	...	Flushed blue.....				
A	5·3 (3)	Yellowish white (3) ...				
B	8·8 (3)	Light blue (3)	AB 46 11 (w 3)	39·84 (w 2)	1862·73	Elchies.

An "optical" pair.

In *Magnitude*, stationary.

In *Colour*, the same; and the difference of tints of the two stars, well established.

In *Position* and *Distance*, showing the effects of the proper motion of A upon B, and to a rather larger extent than indicated in the British Association Catalogue. The latter makes it annually in

$$\text{R. A.} = -0''·08, \text{ and } \text{D} = 0''·00;$$

while the Elchies observation, compared with the mean of STRVUE's and my father's, gives for the same quantity in

$$\text{R. A.} = -0''·09, \text{ and } \text{D} = -0''·05.$$

(7.) 222 ARIETIS, R.A. $1^h\ 51^m\ 56^s$, and D. $+20°\ 23'\ 14''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority
A	8·5	Yellow				
B	11·0	.....	AB 55 32	2·37	1832·42	Σ.
C	9·2	.....	AC 167 22	39·46		
A	6	Topaz yellow	.....	.....		
B	15	Deep blue.....	AB 53 0 (*w* 1)	2·5 (*w* 1)		
C	10	Lilac.....	AC 165 0 (*w* 2)	40·0 (*w* 1)	1834·99	Cycle.
D	9	Pale blue	AD 359 12 (*w* 2)	165·0 (*w* 2)		
A	9	Grey.....	... ..	.....		
B	15	Blue.....	.....	.....	1856·68	Alta Vista.
C	10	Lilac.....	.....	.....		
D	6	Yellow	.....	.....		
A	0 (2)	.....	.....	.....		
B	12 (2)	Bluish (1).....	.....	.....		
C	12 (2)	Bluish (1).....	BC 169 15 (*w* 0·5)	37·26 (*w* 0·3)	1862·74	Elchies.
D	7·5 (2)	Pale yellowish (1).....	BD 0 35 (*w* 0·3)	182·01 (*w* 0·3)		
A	9	.....	.....	.....		
B	10·5	.....	AB 53 42	2·25		
C	9·25	.....	AC 165 36	36·2	1862·9 ?	Dawes.
D	6·25	.....	AD 1 36	182·5		
A	0 (2)	.....	.....	.....		
B	11·5 (2)	Bluish (2)	.....	.....		Lilburn
C	13·5 (2)	Blue (2)	BC 167 32 (*w* 0·5)	36·24 (*w* 0·5)	1862·96	Tower.
D	7 (2)	Yellow (2)	BD 1 23 (*w* 0·4)	181·12 (*w* 0·3)		

Some remarkable changes in *magnitude*, *colour*, *position*, and *distance*, have occurred in this group, according to the authorities. A great writer has indeed asserted, since the Elchies observation was published in the Notices of the Royal Astronomical Society, that the magnitudes and colours assigned to A and D in the Cycle, should be read *vice versâ* for what they are put down. (See Monthly Notices R. A. S. for December 1862.) If this point can be proved, it will greatly alter some of the changes believed to have been experienced; and instead of the Elchies observation, as well as that at Lilburn Tower being interpreted to show that "A" was missing, and "B" only left outstanding, we may then consider that both A and B were seen on each occasion, but indistinguishable the one from the other, by reason of disturbed aerial vision; in which case too, in place of the other positions and distances being entered under the letters BC and BD, they should rather be registered under $\frac{A+B}{2}$ C, and $\frac{A+B}{2}$ D. But granting that alteration, and also the demanded inversion of the Cycle magnitudes and colours for A and D, there still remain as notanda in the group,—

1*st*, The decrease of distance in the last thirty years of A and C;

2*d*, The increase of position angle of A and D;

3*d*, The increase of distance of A and D; and,

4*th*, Most strange of all, the colour of A, as assigned by STRUVE; for he, as well as the Cycle, makes it yellow; and either his recorded colour must be repudiated by the repudiator of the Cycle, or the modern change in that star's colour must be allowed to be real.

The observations now given under the head of Lilburn Tower, are some which I was kindly allowed to make when on a visit last December to my friend, Mr E. J. COLLINGWOOD. His observatory, planned and erected by himself, is a model of perfection in everything which it contains, and is furnished, among other instruments, with an equatorial, whose object-glass, a Munich 6-inch, had been purchased from the late CHARLES MAY, C.E., and by him procured from Mr DAWES, under the statement of its having come out of long and very severe trials for defining power, with the most remarkable success. The mounting is by SIMMS, with some fittings by DOLLOND, in excellent style. Nothing, therefore, was wanting to enable good observations to be made, at least my best, excepting good atmospheric definition. Now both the nights that I observed the object at Lilburn Tower, were stormy in the extreme, and the definition as bad as on the two when I observed it at Elchies with Mr GRANT's larger telescope; and I have described over and over again how bad it was there, in the November Monthly Notices, see pages 3 and 13, as well as indicated the same in the small weights attached to the measures (weights giving much below the smallest weights ever assigned by most other observers to their worst measures). In such a state of atmospheric definition, two small and very close, blue stars, as A and B are now asserted to be, might easily have been confounded together; and I would not have undertaken to decide on the question in that shape at that time *i. e.*, either at Elchies or at Lilburn. But it was not then in that shape to me; for, with the Cycle in my hand, a crowned book, stating, and having stated unquestioned during eighteen years, that the group is "an exquisite object," and "an admirable test to try the light and definition of a telescope;" and describing its three closer members, or A, B, and C, thus:—viz., a large yellow star, A, with one small blue one close to it, and another small blue one a moderate distance off; and then, looking into the telescope, and seeing no trace of a yellow star of any size, big or little, in that place, and only two faint blue burred images there, at just about the distance apart of the two small blue stars, B and C, of the Cycle, —I thought, and still think, myself justified in publishing, that no large yellow star now exists, where the Cycle stated that such a one existed in its day. So far, too, as the subsequent discussion at the Royal Astronomical Society has elicited, the Elchies observation was the first to announce that fact,—a most notable one surely in reference to a "telescopic test object," whether originating in a mistake in the Cycle, or a cosmical change in the starry heavens.

As to the subsidiary question, of whether the faint blue burr seen in both telescopes, in the united place of A and B, might have been really composed of two small and close blue stars, it is no slur on the Elchies telescope's defining power, that that point was not settled then. For, even granting for the moment with the most extreme objectors, that A and B now are, and always have been, these two diminutive and close, blue stars, the power of separating and seeing them as such depends, not only on the defining power of the object-glass, but also on the atmospheric definition at the time and place, as well as on the particular observer's eye. There is no need, however, of confining ourselves to generalities, as there are already recorded in this very case, three sets of observations, all made by one and the same observer, viz., myself; *i. e.*, the Teneriffe, in 1856·68, with a 7·2 object-glass; the Elchies, with an 11-inch; and the Lilburn Tower, with a 6-inch. Of the transcendent defining power of the latter there can be no question, in a good atmosphere; yet, the atmosphere having been as bad at Lilburn Tower as at Elchies, only a single star was, as at Elchies, suspected to exist; but in Teneriffe, 10,000 feet above the sea, where the atmospheric definition was exquisite, two stars were perceived at the first glimpse. The same eye therefore, and a medium size of telescope between the other two, joined to a quality which, though good, could hardly have been superior, but a decidedly better atmospheric definition, produced instantly such superior results. What, therefore, might not be the advantage to astronomy of having a large telescope always mounted in an equally well defining atmosphere to that of the Teneriffian Alta Vista; for evidently five minutes there, on the showing of the objectors themselves, are, for this purpose, better than four whole nights in a worse atmosphere.

(8.) α PISCIUM, R.A. $1^h\ 54^m\ 54^s$; Decl. $+2^\circ\ 5'\ 45''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 337° 23′	5″·12	1779·80	H^1.
A	...	.....				
B	...	.....	AB 333 0	...	1802·08	H^1.
A	...	.....				
B	...	.....	AB 335 33	5·43	1821·93	H^2 and S.
A	3·3	.....				
B	4·8	.....	AB 336 54	3·42	1821·96	σ
A	...	.....				
B	...	.....	AB 332 59 (w 24)	3·775 (w 24)	1830·93	Bessel.
A	2·8	Greenish white				
B	3·9	Blue	AB 335 43	3·64	1831·16	Σ.
A	...	.....				
B	...	.....	AB 331 58 (w 4)	3·760 (w 4)	1834·85	Bessel.

α PISCIUM—*continued.*

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	5	Pale green				
B	6	Blue	AB 333° 24′ (*w* 9)	3″·8 (*w* 9)	1838·87	Cycle.
A	...	.....				
B	...	.....	AB 332 46	3·690	1843·04	Schlüter.
A	...	White				
B	...	White	.....	.....	1844·8	Sestini.
A	...	.....				
B	...	.....	AB 327 58 (*w* 4)	3·750 (*w* 4)	1845·89	Wichmann.
A	...	.....				
B	...	.....	AB 329 30 (*w* 8)	3·50 (*w* 5)	1852·03	Spec. Hart.
A	...	.....				
B	...	.....	AB 328 6	3·42	1853·99	Dawes.
A	...	.....				
B	...	.....	AB 336 56	2·76	1855·72	E. Luther.
A	5·5	.....				
B	6·5	.....	AB 326 53 (*w* 27)	3·18 (*w* 20)	1856·07	Jacob.
A	...	White				
B	...	White	.....	.....	1856·68	Alta Vista.
A	...	.....	AB 327 54 (*w* 14)	.....	1861·18	A. Auwers.
B	...	.....	AB	3·591	1862·14	
A	4·8 (3)	White (2)				
B	6·2 (3)	Bluish white (2)	AB 327 56 (*w* 2)	3·63 (*w* 2)	1862·72	Elchies.

In *Magnitude*, variable through probably a fourth of the whole light.

In *Colour*, slightly variable; A, from white to greenish, and B from the same to bluish.

In *Position* slowly retrograding, and in *Distance* rather decreasing, and not showing relatively the large B. A. C. proper motion of A; therefore in *Character* a slow binary; but, as first pointed out, and much dwelt on by the late Captain Jacob, suffering some large perturbations by an unknown dark body. These, however, will not explain the characteristics of the reputed first-class observation of 1855·72.

(9.) *γ* ANDROMEDÆ, R.A. $1^h\ 55^m\ 25^s$; D. $+41°\ 40'\ 3''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 70° 23′	9″·25	1779·65	H[1].
A	3	Strong golden.....				
B	5	Strong blue.....	AB 62 26	10·33	1830·02	Σ.

γ ANDROMEDÆ—*continued.*

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 62° 17′ (*w* 24)	10″·552 (*w* 24)	1830·76	Bessel.
A	3·5	Orange				
B	5 5	Emerald green.....	AB 61 36 (*w* 9)	11·00 (*w* 9)		
C	5·5	Emerald green.....	BC 120 0 (*w* 1)	0·5 (*w* 1)	1843·33	Cycle.
A	...	Red orange.....				
$\frac{B+C}{2}$	...	Lighter orange.....	.....	.....	1846·5	Sestini.
A	...	.....				
B	...	.....	AB 62 30 (*w* 9)	10·1 (*w* 9)		
C	...	.....	BC 115 0 (*w* 1)	0·5 (*w* 1)	1852·99	Spec. Hart.
A	...	Orange.....				
B	...	Green	.....	.....	1856·68	Alta Vista.
C	...	Green				
A	2·0					
$\frac{B+C}{2}$	5·4	.....	A $\frac{B+C}{2}$ 63 23 (*w* 36)		1860·90	A. Auwers.
		.....	.....	10·270 (*w* 20)	1861·76	
A	3·5(1)	Orange yellow (2)...				
B	5·5(1)	Bluish green (2)	AB 62 12 (*w* 2)	9·81 (*w* 0·3)	1862·71	Elchies.
C	6·0(1)	Bluish green (2).....	AC 116 23 (*w* 0·5)	0·58 (*w* 0·3)		

In *Magnitude*, nearly steady.

In *Colour*, A seems nearly steady; but B, with or without C, rather variable from blue to green at some periods, and purple at others.

In *Position* and *Distance*, A and B nearly steady, B and C with symptoms of orbital movement; but as the tabular proper motion of A does not appear between A and B, they may also be pronounced in *character*, slow binaries.

The discovery of C, one of the many similar triumphs of the acute Otto Struve, with the great Pulkova object-glass, is an admirable test of definition and size of discs. My father's 5·9-inch object-glass would barely notch the B and C pellet of light; Mr Pattinson's 7·25-inch on Teneriffe would separate the two stars and show a dark line between, but still make the discs so large, that they were mutually compressed on their adjacent sides, in order to allow the said dark line to be formed: with the Elchies 11-inch however, the discs were so much smaller that they were both completely separated and perfectly circular, their diameters being, for B = 0″·3, nearly,

and for C = 0″·25, nearly.

But there was this difference caused by the atmosphere of either locality, viz., that at Elchies I only once, and then but for a short time, saw the stars thus defined with all the power of the object-glass; but at Alta Vista, on Teneriffe, I saw them so night after night, and almost whenever I looked at them.

(10.) 32 ERIDANI, R.A. $3^h\ 47^m\ 24^s$; D. $-3°\ 21'\ 52''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 343° 23′	4″·33	1781·81	H^1.
A	...	.....				
B	...	.....	AB 349 1	8·08	1821·90	H^2 and S.
A	4·6	.....				
B	6·4	.....	AB 349 54	6·63	1822·02	σ.
A	4	Strong yellow.....				
B	6	Strong blue.....	AB 347 16	6·70	1833·15	Σ.
A	5	Topaz yellow.....				
B	7	Sea green	AB 346 30 (*w* 8)	6·6 (*w* 7)	1843·16	Cycle.
A	...	Yellow.....				
B	...	White.....	.....	.....	1845·9	Sestini.
A	...	Bright yellow				
B	...	Flushed blue.....	AB 347 0 (*w* 5)	6·8 (*w* 5)	1850·30	Spec. Hart.
A	...	Orange (1)				
B	...	Greenish (1).....	.....	.....	1856·68	Alta Vista.
A	5 (1)	Orange (1).....				
B	7 (1)	Slightly greenish (1)	AB 347 10 (*w* 1)	6·55 (*w* 1)	1862·72	Elchies.

In *Magnitude*, nearly constant.

In *Colour*, brilliant and pronounced; A, nearly always rich yellow; B, rather variable, from blue to greenish, and probably light violet.

In *Position* and *Distance*, constancy nearly prevails, large errors of observation being allowed for. The B. A. C. proper motion of A, if unpartaken of by B, would have decreased the position by 22°, and increased the distance by 1″·3 in 30 years; we may therefore say with confidence that in *character* the pair is a slow binary.

(11.) α HERCULIS, R.A. $17^h\ 8^m\ 20^s$, and D. $+14°\ 32'\ 51''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 111° 28′	4·74	1779·66	H^1.
A	...	.....				
B	...	.....	AB 121 57	5·05	1803·40	H^1.
A	...	.....				
B	...	.....	AB 116 36	5·61	1819·60	σ.
A	...	.....				
B	...	.....	AB 119 33	5·29	1821·74	H^2 and S.

α HERCULIS—*continued.*

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 116° 6′	4″·72	1822·2	σ.
A	3	Egregiously yellow..				
B	6·1	Intensely blue	AB 118 28	4·65	1829·63	Σ.
A	...	.....				
B	...	.....	AB 120 23	4·85	1830·62	Dawes.
A	...	.....				
B	...	.....	AB 118 34 (*w* 30)	4·994 (*w* 30)	1830·92	Bessel.
A	...	.....				
B	...	.....	AB 120 20 (*w* 12)	4·980 (*w* 12)	1833·53	Selander.
A	...	.....				
B	...	.....	AB 119 6	4·94	1834·58	R. Sheep-[shanks.
A	...	.....				
B	...	.....	AB 119 16 (*w* 9)	5·084 (*w* 9)	1835·77	Bessel.
A	...	.....				
B	...	.....	AB 118 40 (*w* 40)	4·955 (*w* 40)	1842·53	Schlüter.
A	3·5	Orange.....				
B	5·5	Emerald	AB 118 42 (*w* 9)	4·5 (*w* 9)	1842·57	Cycle.
A	...	.....				
B	...	.....	AB 123 32 (*w* 12)	4·99 (*w* 12)	1854·61	Luther.
A	3·5	.....				
B	5·5	.....	AB 118 23 (*w* 26)	4·52 (*w* 15)	1856·08	Jacob.
A	...	Orange (1).....				
B	...	Greenish (1).....	.....	.....	1856·68	Alta Vista.
A	...	.....				
B	...	.....	AB 119 24 (*w* 7)	4·8 (*w* 5)	1857·63	Spec. Hart.
A	...	.....				
B	...	.....	AB 117 22 (*w* 34)	4·41 (*w* 17)	1858·04	Jacob.
A	3·3	.....	AB 116 15 (*w* 28)	.....	1861·06	A. Auwers.
B	5·7	.....	AB	5·196	1861·57	
A	3 (1)	Coppery orange (4).				
B	5 (1)	Greenish (4).....	AB 119 30 (*w* 2)	4·71 (*w* 0·8)	1862·72	Elchies.

In *Magnitude*, both stars are known variables through one or two magnitudes.

In *Colour*, they are brilliant, well understood, and constant; a small chromatic equation being allowed for STRUVE's estimate or ideas of blue and green. I have,

indeed, heard of a colour-blind friend—of the orthodox order, which cannot perceive any difference between the brightest red and the most brilliant green—say of another who mistook between some shades of *blue* and green, "how extraordinary! what a stupid fellow* he must be; now, if he had mistaken *red* and green there might have been some excuse for him." Yet the sentiment of the world at large I conclude will be, notwithstanding, that certain varieties of blue and green are very easily mistaken for each other when viewed under varying contrasts with other colours.

In *Position* and *Distance*, these stars are a caution; and in *Character*, who shall say what they are, though, too, they have been so abundantly observed.

"Fixity," relative, from the earliest time, has been strongly claimed for them by some good judges; but in that case, what becomes of the reputed accuracy of the great double star-measurers; and what is the advantage of their occasionally publishing their position angles to the $\frac{1}{100}$th of a minute, and the distances to the $\frac{1}{1000}$ of a second,—when a discrepance of no less than five whole degrees in position, and half a second in distance, is shown, by thus bringing together various records of α Herculis, to be a very common error for a long and well weighted series of observations. What, also, at this rate, is likely to be the value of a double-star orbit, computed on a small number of observations extending over no more than a very moderate portion of its periodic time?

Granting, however, for argument, the imputed relative fixity, these stars would become, under the B. A. C. proper motion test, "slow binaries;" seeing that their "position" should otherwise have increased twenty-one degrees in the last thirty years.

But then, again, other authorities have challenged the truth of the B. A. C. proper motion in this case, which they considered extremely erroneous; and the late Captain Jacob, when at Madras, undertook a series of most carefully conducted equatorial observations, to ascertain whether the group was, in *character*, optical or physical. These observations have been fully printed by the Royal Astronomical Society, and resulted in showing with remarkable force, in as far as the agreement of the observations, *inter se*, could make it, that the group was not only *optical*, but that the annual parallax of one member upon the other was sensible, and even well determined in its amount from the measures. Yet its full effect, only reached, between its two six-monthly extremes, to one-fifth of the difference between his *mean* of measures and that of Dr Luther taken at nearly the same epoch; while Dr A. Auwers, on the mean of four observations on each night, has in the year 1861 a difference, reaching, in the short space of four nights, to three times the whole extent of Captain Jacob's imputed annual parallactic effect.

* It was not M. Struve, nor indeed any astronomer whatever, who was thus referred to.

Had these several able and learned astronomers observed in the pure, transparent, and definition-favouring air of the upper parts of the Peak of Teneriffe, or a similar air in any other part of the world,—I feel sure they would not have racked all men with anxieties, and covered the nature of this star with the confusion arising from such impossible numerical quantities.

(12.) ξ HERCULIS, R.A. $17^h\ 18^m\ 56^s$, and D. $+37°\ 16'\ 35''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 300 21	2·97	1779·66	H^1.
A	...	.....				
B	...	.....	AB 306 0	3·97	1822·4	σ.
A	4	Greenish white.....				
B	5·1	Greener.....	AB 307 13	3·60	1830·35	Σ.
A	4	Bluish white..... ..				
B	5·5	Pale emerald.....	AB 308 54 (*w* 9)	3·7 (*w* 9)	1839·74	Cycle.
A	...	Yellow				
B	...	Deeper yellow	.....	.....	1844·4	Secchi.
A	...	Greyish.....				
B	...	Greenish	.....	.....	1850·5	Spec. Hart.
A	...	.....				
B	...	.....	AB 310 30 (*w* 7)	3·5 (*w* 4)	1853·39	Spec. Hart.
A	...	White (1).....				
B	...	Bluish (1)	.....	.....	1856·58	Guajara.
A	4 (1)	Whitish yellow (2)				
B	6 (1)	Yellowish grey (2)	AB 310 38 (*w* 1)	3·62 (*w* 0·8)	1862·72	Elchies.

In *Magnitude*, steady.

In *Colour*, variable; A varying from white to bluish, greenish, and yellow; while B varies from bluish to green, grey, and yellow.

In *Position* and *Distance*, "binary" character is confirmed; yet so slow a binary, that it had long been looked on as relatively fixed, and therefore optical, when micrometer measures were alone looked to. Had, however, the B. A. C. proper motion test been applied, the true result might have been deduced much earlier; for that shows how the stars should have *decreased*, instead of increased, their angle of position, and by so much as 18° in thirty years, had they been optically connected only.

(13.) 95 HERCULIS, R. A. $17^h\ 55^m\ 38^s$, and Decl. $+21°\ 35'\ 55''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 265° 51′	6″·10	1780·69	H^1.
A	4·5	Greenish yellow				
B	4·5	Reddish yellow	AB 262 30	6·19	1828·71	Σ
A	5	Greenish				
B	5	Yellowish	AB 262 12	6·11	1828·76	Σ.
A	5	Wonderful yellow gr.				
B	5	Egregiously red.....	AB 261 12	6·07	1829·62	Σ.
A	5	Yellower				
B	5	Yellow	AB 261 6	5·88	1832·53	Σ.
A	5·5	Light apple green...				
B	6·0	Cherry red	AB 261 48(w 9)	6·1(w 9)	1833·78	Cycle.
A	...	Gold yellow				
B	...	Gold yellow	.....	.....	1844·5	Sestini.
A	...	Pale green.....				
B	...	Reddish.....	.....	.....	1851·3	Spec. Hart.
A	...	Greyish white(2) ...				
B	...	Greyish white(2) ...	.....	.....	1856·58	Guajara.
A	...	Light green				
B	...	Pink	AB 261 11	6·15	1857·42	Fletcher.
A	...	Greenish				
B	...	Reddish.....	AB 260 11	6·09	1857·45	Wrottesley.
A	...	Light Apple green...				
B	...	Cherry red.....	AB 260 30(w 7)	5·6(w 6)	1857·63	Spec. Hart.
A	5·7(1)	Yellowish with blue tinge(3)	AB 261 4(w 1)	6·29(w 0·6)	1862·72	Elchies.
B	6·0(1)	Slightly reddish yellow(3) ...				

In *Magnitude*, slightly variable; in *Colour*, excessively variable and periodically! This last is both an extreme conclusion and a new one, yet is, I hope, justifiable by the data, and true in nature. The stars are on the average, it will be seen, always diversely coloured, one greenish and the other reddish; and, as at Elchies I saw them of these tints, I am not colour-blind to them; yet when I was in Teneriffe, on Mount Guajara, observing with the 3·6 inch Sheepshanks telescope, the stars appeared to me, and on two nights, to be of undistinguishably the same tint; some flashes or thoughts of colour were fancied at times, but they were over

both stars equally, and the mean for each of them was little else, or did not amount to anything more decided, than "greyish-yellow."

Such then was the equality of nameless colour which I felt bound to assign to the stars on two nights, though I was comparing them with the Cycle, and the Cycle only, and the author of that standard book describes A, as being of a "Light apple-green" colour, and B, "cherry red." On informing my Father of these results, he called together a number of his observing friends in the following summer, and, as detailed in the "Speculum Hartwellianum," p. 317, they again made one star "green" and the other "red."

My Teneriffe observations therefore seemed utterly wrong, and without any precedent, except that SESTINI's observations in 1844·5, and given at p. 313 of the same work, made them both "gold yellow." But might he be colour-blind, seeing that the reputed tints are precisely those which are confounded by persons with that peculiarity of vision? Or again, seeing that both he, and myself on Guajara, having given nothing but the colours, and not having identified the star-group beyond all question by making micrometrical observations at the same time of "Position" and "Distance,"—might we both have got hold of a wrong star-group? I could not say anything formerly to these questions, except that no such suspicion crossed my mind on either of the two nights at Teneriffe, and when more difficult stars still, were found; but a new view of the whole case has opened up, on referring to STRUVE's original and individual observations in the "Mensuræ Micrometricæ."

In his mean result for the pair, he agrees nearly with all the rest of the world, by calling one star "greenish-yellow," and the other "reddish-yellow;" but appended to his single observations we find as follows,—

In 1828·71 A is Greenish-yellow, and B Reddish-yellow.
In 1828·76 A is Greenish, and B Yellowish.
In 1829·62 A is Yellow-green and B Egregiously red,
with the note that the colours are "wonderful."

But in 1832·53 A is simply described as "yellower" than B; from which it would seem that they were then both yellow; but one, a more pronounced hue of that colour than the other.

Now, on each of these four occasions, STRUVE measured both "Position" and "Distance," thereby proving the identity of the star-group. He also, three times over, demonstrates himself capable of distinguishing, or being affected by, green and red colours, even to an extreme degree; and yet this same, and most able man, with the same powerful telescope on the fourth and last occasion, sees these distinguishing colours gone, and the two stars reduced to an almost equality of colour. A case so observed cannot be slighted, and it forms the third occasion (the first in point of date) on which such a phenomenon has been noticed with 95 Herculis; let us inquire what interval of time separates the manifestations:—

STRUVE's occurred in	1832·53
SESTINI's occurred in	1844·5
My own occurred in	1856·58

or, a period of twelve years, exact almost to the tenth of a year, separates each of these apparent anomalies. Unless then some most extraordinary combination of errors has occurred, we have stumbled here on a perfectly unexampled case of periodical change of colour in two stars, and a change which, in the course of its cycle, goes through most conspicuous phases.

So far as I know, this conclusion for the stars is quite new; STRUVE, did indeed, in his note at p. 98 of the "Mensuræ Micrometricæ," remark something entirely unusual affecting these stars, and distinguishing them from all others that he ever observed; and I cannot but think that he meant to allude to something more than the mere average diversity of their colours one from the other, for that is not an unusual feature amongst double-stars; but what it may have been, does not appear.* At all events he does not make, and at that period had not observations to give him any hint or clue to a periodic time of change.

In *Position* and *Distance*, something is also determinable from our observations: for even so late as 1860, the "Spec. Hart.," a high authority, speaks of the established "fixity of this optical pair." Yet a glance at our collected table of results, beginning with Sir WILLIAM HERSCHEL, and ending with Elchies, sufficiently demonstrates that a slow retrograde movement in position angle is going on, at the rate of about 2° in fifty years, without any sensible change of distance; a feature at once of true *binary* movement. This character too, is further confirmed by the B. A. C. proper motion test; for according to that, if the stars were optically connected only, the position ought to have decreased 14°, and the distance increased 1″, in thirty years, which is absurd. Our new conclusion therefore of *binary* character for this pair seems confirmed, and is of infinite importance in considering the periodical variations of colour which its members undergo.

(14.) 70 OPHIUCHI, R.A. $17^h\ 58^m\ 28^s$, D. $+2°\ 32'\ 32''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 90° 0′	3″·59	1779·77	H^1.
A	...	.....				
B	...	.....	AB 318 48	2·56	1804·41	H^1.

* STRUVE's note is as follows:—

"Colorum diversitas ex singulorum dierum consensu indubia est. Probe notandum est has colorum notationes sine ullo præjudicio esse factas, cum in tot stellis duplicibus observandis occupatus, nunquam recordarer, quid peculiaris in hoc illave notaverim. Eadem colorum relatio etiam a H^2 et S. est reperta, scilicet priorem esse flavescentem, posteriorem non. Eadem denique in observationibus per instrumenta meridiana sæpius annotavi. Insignis est differentia luminis in stelli ejusdem proxime splendoris."

70 OPHIUCHI—*continued.*

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	4·1	Yellow				
B	6·1	Purple	AB 148 13′	3″·98	1825·57	Σ.
A	5	.....				
B	8	.....	AB 129 18 (*w* 7)	6·98 (*w* 10)	1836·7	H_2.
A	4·5	Pale topaz				
B	7	Violet	AB 122 24 (*w* 9)	6·64 (*w* 6)	1842·55	Cycle.
A	...	Gold yellow				
B	...	Gold yellow	.....	...	1845·9	Sestini.
A	...	.....				
B	...	.....	AB 119 42 (*w* 6)	6·80 (*w* 5)	1847·48	Spec. Hart.
A	...	Topaz yellow				
B	...	Purplish	.....	...	1849·5	Spec. Hart.
A	...	.....				
B	...	.....	AB 114 54 (*w* 8)	6·50 (*w* 5)	1852·44	Spec. Hart.
A	...	Pale yellow (1)				
B	...	Greenish (1).....	.....	...	1856·58	Guajara.
A	4·1	.....				
B	6·1	.....	AB 105 59 (*w* 22)	6·209 (*w* 22)	1861·74	Auwers.
A	5(1)	Yellow (4)				
B	7(1)	Light Indian red (4)...	AB 108 28 (*w* 2)	6·12 (*w* 0·8)	1862·73	Elchies.

A celebrated binary star of about ninety years' period, and with perturbations from a supposed large accompanying planet.

In *Magnitude*, steady; in *Colour*, A, constant; but B, variable through yellow, green, violet, purple, and red, though in a period not yet ascertained.

(15.) η SERPENTIS, R.A. $18^h\ 14^m\ 10^s$, D $-2°\ 55'\ 36''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 99° 7′	81″·04	1780·47	H_1.
A	4	Golden yellow				
B	13	Pale lilac	AB 76 54 (*w* 5)	110·9 (*w* 3)	1835·61	Cycle.
A	3·3	Yellow				
B	12·0	.....	AB 77 11	112·70	1836·46	Σ.
A	4 (1)	Yellow (1)				
B	12 (1)	Bluish (1)	AB 70 15 (*w* 0·8)	133·16 (*w* 0·6)	1862·74	Elchies.

In *Character*, an optical pair, experiencing much proper motion relatively.

Comparing the mean of the Cycle and Σ with the Elchies observation, and computing therefrom the annual proper motion of A upon B, it comes out

In R.A. − 0″·60, and in D. + 0″·76;

the British Association quantity for A being

In R.A. − 0″·56, and in D. + 0″·65.

(16) α LYRÆ, R.A. 18h 32m 14s, and D. + 38° 39′ 8″, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...					
B	...	.....	AB 116° 14′	42″·99	1792·32	H1.
A	...					
B	...	.....	AB 132 7	42·11	1822·67	H2 and S.
A	1	Bluish white.....				
B	10·5	.....	AB 137 51	42·96	1836·14	Σ.
A	1	Pale sapphire ...				
B	11	Smalt blue	AB 140 18 (*w* 9)	43·4 (*w* 9)	1843·34	Cycle.
A	...	White				
B	...	Violet	.....	.....	1856 58	Guajara.
A	1 (2)	White, with spurious outer violet rays (3)				
B	11 (1)	Reddish lilac (3)	AB 149 38 (*w* 2)	45·91 (*w* 1)	1862·73	Elchies.

In *Character*, an optical pair, influenced relatively by proper motion.

In *Magnitude*, probably steady; but I could say little else of A at Elchies, than that it was the most gorgeous, glorious, and brilliant star I ever had the privilege of beholding in a telescope, and there was nothing else with which I could approximately compare it.

In *Colour*, they may also be steady; for, as to the reddening appearance of B at Elchies, it may have been an ocular effect, in deference to the spurious outside violet rays of the great star A. (See p. 374.)

In *Position* and *Distance*, the Elchies measure being compared with the mean of STRUVE'S and the Cycle, gives for the annual proper motion of A upon B

In R.A. + 0″·28, and in D. + 0″·30,

while the British Association Catalogue gives for A

In R.A. + 0″·30, and in D. + 0″·28.

an unexpectedly close approach.

(17.) 28 AQUILÆ, R.A. $19^h\ 13^m\ 14^s$, and D. $+12°\ 7'\ 22''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 176° 54′	60″·01	1822·19	σ.
A	6	Pale white				
B	10	Deep blue.....	AB 175 42 (*w* 8)	59·8 (*w* 5)	1831·42	Cycle.
A	...	White				
B	...	Yellow	.....	.....	1844·5	Sestini.
A	...	Dusky white.....				
B	...	Lilac blue.....	.....	.....	1851·4	Spec. Hart.
A	6·2 (3)	Yellowish (1) ...				
B	10·0 (3)	Purplish (1).....	AB 176 38 (*w* 1)	60·97 (*w* 0·6)	1862·72	Elchies.
C	17·0 (2)	Bluish (1).....	AC 102 25 (*w* 0·4)	67·35 (*w* 0·1)		

In *Character*, uncertain, owing to the paucity of observations yet made; and the new determination of the proper motion of A.

The Elchies telescope picked up and observed a new star, C, which may be useful in determining the proper motion of A or B, or both.

(18.) δ AQUILÆ, R.A. $19^h\ 18^m\ 31^s$, and D. $+2°\ 50'\ 25''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	3·5	White				
B	12·0	Livid	AB 259° 18′ (*w* 1)	96″·5 (*w* 1)	1833·62	Cycle.
A	3·5	.....				
B	0	Not seen	.....	.....	1856·58	Guajara.
C	14	.....				
A	3·5 (2)	Faint yellow.....				
B	16 (2)	Bluish	AB 275 35 (*w* 0·3)	95·78 (*w* 0·2)	1862·74	Elchies.
C	14 (2)	Bluish	AC 114 15 (*w* 0·3)	193·94 (*w* 0·2)		

In *Character*, uncertain at present.

In *Magnitude*, A seems steady; but B, both by the Guajara and the Elchies observation, has decreased; while C may be constant.

In *Position* and *Distance*, the Elchies observations do not confirm the B. A. C. proper motion test for A; but then they are excessively rude and little trustworthy, and future observers of A should measure it carefully both from B and C.

(19.) 128 ANSERIS, R.A. $19^h\ 20^m\ 26^s$, and D. $+19°\ 37'\ 12''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority
A	5·5	Egregiously golden..				
B	10·3	.....	AB 43° 35′	22″·65	1829·40	Σ.
A	6·5	Topaz yellow.....				
	13	Deep blue.....	AB 44 48 (*w* 3)	25·0 (*w* 1)	1833·58	Cycle.
A	6·5	.....				
B	13	.....	AB 45 estim.	25 estim.		
C	11	.....	AC 320 estim.	70 estim.	1856·58	Guajara.
A	6·5 (2)	Orange (1)				
B	13·0 (2)	Violet (1).....	AB 41 2 (*w* 1)	22·73 (*w* 0·3)		
C	12·0 (2)	Blue (1)	AC 319 55 (*w* 1)	68·7 (*w* 0·2)	1862·71	Elchies.

In *Character* still uncertain, from the scantiness and rudeness of the observations.

(20.) β CYGNI, R.A. $19^h\ 25^m\ 9^s$, and D $+27°\ 40'\ 20''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 54° 52′	34″·83	1782·45	H.
A	...	.....				
B	...	.....	AB 54 31	34·28	1800·00	P.
A	...	.....				
B	...	.....	AB 54 30	34·29	1821·76	σ in C.
A	...	.....				
B	...	.....	AB ...	34·81	1822·72	σ.
A	...	.....				
B	...	.....	AB 54 45	34·38	1822·98	H^2 and S.
A	...	.....				
B	...	.....	AB 55 32	34·51	1830·54	Dawes.
A	...	.....				
B	...	.....	AB 55 24	34·20	1830·81	Cycle.
A	...	.....				
B	...	.....	AB 55 38 (*w* 28)	34·327 (28)	1831·81	Bessel.
A	3·0	Yellow				
B	5·3	Blue.....	AB 55 44	34·29	1832·18	Σ.
A	3	Topaz yellow				
B	7	Sapphire blue	AB 55 36 (*w* 9)	34·40 (*w* 9)	1837·58	Cycle.

β CYGNI—*continued.*

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.. ...				
B	...	.....	AB 55° 24′ (*w* 40)	34″·554 (*w* 40)	1842·88	Schlüter.
A	...	Orange gold....				
B	...	Azure..... ...	.....	.. ...	1844·5	Sestini.
A	...	Golden yellow.				
B	...	Smalt blue.....	.....	.....	1849·6	
A	...	.....				
B	...	.....	AB 56 12 (*w* 8)	34·10 (*w* 5)	1854·67	Spec. Hart.
A	...	.....				
B	...	.....	AB 55 12 (*w* 12)	33·872 (*w* 12)	1855·84	E. Luther.
A	...	Pale yellow.....				
B	...	Blue	.....	.....	1856·59	Guajara.
A	...	Rich yellow.....				
B	...	Brilliant blue	AB 55 25	34·37	1857·42	Fletcher.
A	...	Golden yellow....				
B	...	Greenish blue.....	AB 55 16	34·56	1857·47	Wrottesley.
A	3·0	.....				
B	5·0	.....	AB 55 20 (*w* 20)	34·383 (*w* 20)	1861·76	Auwers.
A	3·0	Yellow (3).....				
B	7·0	Bluish green (3)..	AB 55 28 (*w* 2)	34·50 (*w* 1)		
C	15·3	Blue (3).....	AC 340 2 (*w* 1)	62·03 (*w* 0·5)		
D	15·0	Blue (2).....	AD 33 45 (*w* 0·5)	108·44 (*w* 0·2)	1862·72	Elchies.

In *Magnitude*, B is supposed to be slightly variable.

In *Colour*, there would seem to be yellow pulsations; at their maximum converting the single yellow of A into golden yellow, and the blue of B into greenish blue, and even bluish green.

In *Position* and distance these stars present one of the most remarkable instances ever met with of near conformity between all ages, countries, observers, and instrumental methods. Only once, in the whole series, does the second of "distance" alter; and only once, excepting in the earlier examples when there may be a cosmical reason for it, does the degree of "Position" change!

In *Character*, the pair may be a slow binary; for though the B. A. C. proper motion test has barely sensible effects, yet they are precisely opposite to what seems to have occurred; *i.e.*, they should have kept the Position constant, but decreased the distance by 1″ in thirty years; but in place of that the Position seems sensibly to have increased, in sixty years at least, and the distance to have remained constant.

The two small stars picked up at Elchies may be useful in future inquiries into these conditions.

(21.) ζ SAGITTÆ, R.A. $19^h\ 42^m\ 52^s$, and D. $+18°\ 47'\ 57''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 304° 10′	8″·83	1781·88	H_1.
A	5·2	.....				
B	9·7	.....	AB 309 15	8·58	1822·65	σ.
A	...	.....				
B	...	.....	AB 314 32	8·82	1823·69	H_2 and S.
A	...	.....				
B	...	.....	AB 318 26	9·81	1829·63	H_2
A	5·7	Green white				
B	8·8	Blue	AB 312 50	8·49	1831·10	Σ.
A	5	Silvery-white.....				
B	9	Blue.....	AB 312 18 (w 9)	8·6 (w 9)	1838·67	Cycle.
A	...	Yellowish-white.....				
B	...	Azure	.....	.....	1844·5	Sestini.
A	...	Yellow (1).....				
B	...	Blue(1).....	.....	.....	1856·68	Alta Vista.
A	5·2 (3)	Whitish (3).....				
B	9·7 (3)	Violet (3)	AB 313 19 (w 2)	8·66 (w 1)		
C	15·0 (3)	Blue (3).....	AC 250 42 (w 1)	71·35 (w 0·5)	1862·73	Elchies.

In *Magnitude*, a variation of one magnitude seems probable.

In *Colour*, A variable from white to yellowish; but B constant.

In *Position* and *Distance* these stars have been a sore puzzle ever since their first discovery, and are nearly a converse to β Cygni, for they form a pair of which hardly any one succeeded during half a century in obtaining a tolerably accordant measure. Differences, indeed, of 14° in angle of position, and 1″·5 in distance, seem to destroy all our accustomed faith in the skill of double star-observers and our belief in the superior closeness or accuracy of their measures: yet all these symptoms of movement of some sort, notwithstanding, one of the most recent (1860) standard published authorities describes the true conclusion to be that the stars are relatively "fixed," and in "optical relationship beyond all reasonable doubt."

This conclusion is however negatived by the B. A. C. test of proper motion, which in such case would demand a decrease of position to the extent of 21°, and increase of distance to 1″·2 in thirty years, a result which the Elchies observation declares emphatically has not taken place. A and B should therefore from this

indication be slow binaries; and the micrometer measures fully considered would appear to tend towards the same conclusion, with the addition that the motion is *direct* on the whole, but largely perturbed by an accompanying dark body. This latter interesting result is somewhat strengthened by reference to STRUVE's individual observations, which are thus:—

Pos.	Dist.	Date
312° 12′	8″·78	1828·71
312 30	8·71	1828·72
314 0	8·29	1829·84
313 36	8·15	1832·76
312 30	8·43	1832·79
312 12	8·50	1833·78

And where, though the discordance of the H_2 observation of 1829·63 cannot be altogether explained, yet the mass of it is seen to occur agreeably with STRUVE's largest position. No such large disturbances have been observed lately, it is true; but then, as the period of perturbations would seem likely to be something very short, as five or ten years, it is quite possible that the crucial points of the orbit have been passed over, unattended to, so few and far between have been the modern observations.

(22.) α AQUILÆ, R.A. $19^h\ 44^m\ 2^s$, and Decl. $+8°\ 30'\ 5''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 334° 4′	1″(4)3·40	1781·56	H^1
A	...	.....				
B	...	.....	AB 326 6	153·71	1821·85	σ
A	...	.....				
B	...	.....	AB 325 48	153·37	1823·11	H_2 and S.
A	1·5	Pale yellow.....				
B	10·0	Violet tint	AB 323 6 (w8)	152·6 (w 5)	1834·81	Cycle.
A	1·5	Yellowish white				
B	10·2	.....	AB 322 6	152·37	1836·29	Σ.
A	...	Pale yellow (1)				
B	...	Grey (1)	.....	...	1856·68	Alta Vista.
A	1 (2)	Yellowish white (2) ..				
B	10·5 (2)	Reddish white (2)....	AB 316 0 (w1)	155·75 (w 0·5)	1862·73	Elchies.

In *Magnitude*, probably steady.

In *Colour*, also probably steady, the apparent reddening of the companion being explainable under the hypothesis given at pages 374 and 402.

In *Position* and *Distance*, some uncertainties have hitherto prevailed. It is a

difficult pair no doubt to observe, on account both of the difference of size in the components and their great distance apart; and some error, perhaps a typical one, seems included in the H_1 distance. But putting that on one side, there is running through the whole period, a large and rapid change of position angles with very little change of distance. Now this would in most cases be at once put down as indicative of the distinguishing features of *binary*, as contrasted to *proper*, motion: but in that case what an angular subtense for a stellar orbit, something like 5′ of space altogether; or 5 times as large as the utmost stretch ever given to α Centauri, hitherto known as the largest orbit presented by any star-system in either Northern or Southern hemisphere of the sky. The case therefore requires caution; while both the Cycle in 1844, and the Spec. Hart. in 1860, think that the question is still an open one; and at the latter date, that "years must yet elapse" before it can be either confirmed or contradicted.

The Elchies observation however helps us to a conclusion at once; for, taking that as the later date, and for the earlier one the mean of the four sets given between 1821 and 1836, which must surely give a good determination for its mean epoch, and computing thence the annual proper motion, we find that it chances to be in such a direction as to alter the position much and the distance little, giving finally for the proper motion of A on B in a straight line,

In R.A. + 0″·57 and Decl. + 0″·36,

while the absolute proper motion of A, as determined by Meridian Instruments, and recorded in the British Association Catalogue, is

In R·A. + 0″·51, and Decl. + 0″·38.

So remarkably near an agreement as this, must, I think, be held to settle now beyond all cavil, that—

In *Character*, this is an optical pair only.

(23.) γ DELPHINI, R. A. $20^h\ 40^m\ 17^s$, and Decl. + 15° 37′ 54″, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 274° 9′	11″·82	1779·74	H-.
A	...	.....				
B	...	.....	AB 273 0	13·50	1800	P.
A	...	.....				
B	...	.....	AB 273 26	11·83	1823·34	σ
A	...	.....				
B	...	.....	AB 273 43	12·31	1823·68	H^2 and S.

γ DELPHINI—(*continued*).

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 273° 20′	12″·02	1830·57	Dawes.
A	...	Golden				
B	...	Bluish Green.....	AB 273 45	11·90	1830·89	Σ.
A	...	.....				
B	...	.....	AB 272 53(*w* 28)	12·016 (*w* 28)	1830·89	Bessel.
A	...	.....				
B	...	.....	AB 273 7 (*w* 24)	12·045 (*w* 24)	1834·76	Bessel.
A	4	Yellow				
B	7	Light emerald	AB 273 18 (*w* 8)	11·8 (*w* 7)	1839·71	Cycle.
A	...	.....				
B	...	.....	AB 272 48(*w* 40)	11·943 (*w* 40)	1842·63	Schlüter.
A	...	Orange				
B	...	Yellow	.....	.....	1844·45	Sestini.
A	...	Golden yellow				
B	...	Flushed grey.....	.....	.....	1850·7	Spec. Hart.
A	...	.....				
B	...	.....	AB 272 55 (*w* 4)	11·530 (*w* 4)	1853·83	Peters.
A	...	.....				
B	...	.....	AB 273 8(*w* 20)	11·521 (*w* 20)	1856·52	Luthers.
A	...	Cadmium yellow (1)				
B	...	Greenish tinge (1)..	.....	.....	1856·68	Alta Vista.
A	3·4					
B	4·9	.....	AB 272 5(*w* 30)	.....	1861·07	Auwers.
				11·806 (*w* 22)	1861·38	Auwers.
A	4·5 (2)	Pale yellow (2).....				
B	7 (2)	Slightly greenish (2)	AB 272 45 (*w* 1)	11·15 (*w* 0·3)	1862·72	Elchies.

In *Magnitude*, not quite constant.

In *Colour*, both A and B seem to undergo yellow pulsations.

In *Position* and *Distance*, there is much still unsettled. STRUVE expressly says, no alteration in angle: and the Cycle, decides on "fixity." But Dr A. AUWERS claims a small decrease of distance; and I think that he is not only right there (though the Elchies observation is probably a very bad one), but that the same change has been taking place in Position. In the British Association Catalogue, proper motions to a considerable extent, and nearly in the same direction, are attributed to either star; this looks like a binary connection, and if the difference of those proper motions is not exactly borne out by the Positions and Distances observed, we can at least say, that the whole proper motion of one most assuredly

does not display its effect on the other, as though it were distant and in optical connection only. Hence,

In *Character*, this pair is probably a slow binary, and with its orbit viewed at a small angle.

(24.) 452 CYGNI, R. A. $20^h\ 57^m\ 4^s$, and Decl. $+38°\ 58'\ 0''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	7	Deep yellow				
B	11	Emerald hue	AB 297° 0′ (*w* 3)	17″0 (*w* 1)	1832·87	Cycle.
A	6 (2)	Reddish orange (2)				
B	10 (2)	Greenish (2)	AB 300 33 (*w* 2)	19·39 (*w* 1)		
C	13 (2)	Bluish (2)	AC 86 53 (*w* 1)	98·87 (*w* 0·4)	1862·73	Elchies.
D	15·5 (2)	Bluish (2)	AD 138 54 (*w* 1)	54·76 (*w* 0·2)		

Further observations should be accumulated before any conclusions are ventured on, as to the character of this group. At present it merely offers an example, by the two additional stars introduced, of the happiness of reobserving an ancient stellar object with a larger telescope than before employed upon it.

(25.) 1 PEGASI, R.A. $21^h\ 15^m\ 43^s$, and D. $+19°\ 12'\ 56''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 308° 19′	37″10	1780·69	H[1].
A	4	.....				
B	9·5	.....	AB 310 0	($\Delta\delta$ 24·70)	1823·87	σ.
A	4	Pale orange				
B	9	Purplish	AB 310 48 (*w* 8)	36·4 (*w* 5)	1833·95	Cycle.
A	4·5	Strongly yellow.....				
B	8·6	.....	AB 311 15	36·20	1835·86	Σ.
A	...	Orange				
B	...	Azure	.....	.....	1844·5	Sestini.
A	...	Deep yellow				
B	...	Lilac blue.....	.....	.....	1851·4	Spec. Hart.
A	...	Rich yellow (2) ...				
B	...	Purplish blue (2) ...	.....	.....	1856·68	Alta Vista.
A	4·5 (2)	Yellowish (3)				
B	9·0 (2)	Grey (3)	AB 311 16 (*w* 2)	37·09 (*w* 0·5)		
C	14·5 (2)	Blue (3)	AC 22 59 (*w* 1)	80·73 (*w* 0·3)	1862·72	Elchies.

In *Magnitude*, nearly steady.

In *Colour*, the same.

In *Position* and *Distance*, showing some binary character, which is confirmed by the B. A. C. proper motion test. This should, however, be confirmed equatorially by future measures of the Elchies star C.

(26.) 3 PEGASI, R.A. $21^h\ 30^m\ 51^s$, and D. $+6°\ 0'\ 2''$, January 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	...	.....				
B	...	.....	AB 352° 48′	34″·72	1782·76	H_1.
A	...	.....				
B	...	.....	AB 350 48	36·90	1800·00	P.
A	...	.....				
B	...	.....	AB 350 30	39·21	1821·54	σ.
A	...	.....				
B	...	.....	AB 348 58	39·52	1823·79	H_2 and S.
A	6	White				
B	7·4	White	AB 349 26	39·15	1834·91	Σ.
A	6	White				
B	8	Pale blue	AB 349 30 (*w* 8)	39·3 (*w* 8)	1837·81	Cycle.
A	...	White				
B	...	Yellow	.....	.....	1844·5	Sestini.
A	...	Flushed white				
B	...	Greyish	.....	.....	1850·5	Spec. Hart.
A	...	Whitish (1)				
B	...	Warm grey (1).....	.....	.....	1856·68	Alta Vista.
A	6	(3) Greyish green (3)				
B	7·3	(3) Lilac tinge (3)	AB 350 19 (*w* 1)	41·69 (*w* 0·3)	1862·73	Elchies.

In *Magnitude*, steady.

In *Colour*, A nearly steady; but B, very variable.

In *Position*, nearly constant, but in *Distance*, undergoing a sensible increase: more observations are however needed to confirm the conclusion.

(27.) 312 PEGASI, R.A. $21^h\ 45^m\ 6^s$; Decl. $+19°\ 10'\ 53''$, Jan. 1, 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	6·5	White.....				
B	14·0	Blue.....	AB 95° 0′ (*w* 1)	15″·0 (*w* 1)	1838·66	Cycle.
A	7	(2) Yellowish (1).....				
B	15	(2) Bluish (1).....	AB 92 28 (*w* 1)	20·49 (*w* 0·5)		
C	17	(2) Bluish (1).....	AC 323 34 (*w* 0·5)	21·94 (*w* 0·3)		
D	15·5	(2) Bluish (1).....	AD 193 48 (*w* 0·4)	81·66 (*w* 0·2)	1862·74	Elchies.
E	17	(1) Bluish (1).....	AE 355 52 (*w* 0·2)	128·40 (*w* 0·1)		
F	18	(1) Bluish (1).....	AF 308 52 (*w* 0·2)	107·00 (*w* 0·1)		

The Elchies telescope was turned upon this object, because the description in the Cycle seemed so pointedly to define when the Cycle telescope stopped in making out small stars,—viz. decidedly below Sir JOHN HERSCHEL's 20-foot reflector,—that it seemed an excellent opportunity for ascertaining whereabouts the Elchies telescope, worked by an eye very probably much inferior to both the others, would come.

The Cycle describes the group as "a most delicate double star *in a barren field*; and that it "is No. 947 of H's 20-foot sweeps, and classed triple, having a minute *comes* in the *np*, of the 17th magnitude. But this I '(Cycle)' could not manage to get a sight of, notwithstanding the coaxings exerted."

The Elchies telescope, however, not only showed the above double-star, but also Sir JOHN HERSCHEL's C, still existing in the relative place which he pointed out; and in addition to that, several other and previously unrecorded stars, as the D, E, and F of our list; and the truth of their finding has been since testified to in an uncomplimentary, but, for the truth of a scientific fact, a most unexceptionable manner, by the Rev. W. R. DAWES, in the Astronomical Society's Monthly Notices for December 1862, page 79, paragraph 3.

(28.) 2841 Σ, R.A. $21^h\ 47^m\ 46^s$, and Decl. $+19°\ 2'\ 53''$, Jan. 1. 1862.

Components.	Magnitude.	Colour.	Position.	Distance.	Date.	Authority.
A	6·5	Conspicuously yellow..				
B	8·0	Blue.....	AB 111° 2′	22″·21	1829·46	Σ.
A	6·7(3)	Yellow(3).....	.....	.....	.....	.....
B	8·7(3)	Lilac(3).....	AB 110 58 (*w* 2)	22·46 (*w* 1)	1862·73	Elchies.

The previous star, No. 27, the penultimate of our list, gave some fair indications of the abundant light-transmitting power of the Elchies telescope; and the present star, the ultimate member of the same list, happens curiously enough to close our little record of the trial of Mr GRANT's large telescope, with data having reference possibly to the power of accuracy or agreement with standard authorities in each of the four telescopic phenomena usually noted with every considerable double star.

The case would of course be infinitely more satisfactory, if there had been several observations by earlier authorities to refer to, and prove thereby whether this was a stationary or changing group; but I knew of none such at the time, and did not even know of this one until after I had left Elchies, and had communicated my own measures to my friend, Professor GRANT, of the Glasgow Observatory,—and he discovered that they must apply to STRUVE's star, No. 2841.

The whole case occurred in this manner. I had been so long one evening writing down some of the particulars observed about the star 312 Pegasi, that when I went back to the telescope the equatorial driving clock had run down, and the star was gone. But on looking through the chief finder, lo! there was in its place

a different and a conspicuous double star near the boundary of the field; and on bringing this stranger into view with the great telescope, it proved quite a splendid object, which I measured, as recorded above; and was much surprised to find that there was no mention whatever of its existence in the "Cycle," *which was my only book of reference at the time.* I wrote, therefore, to Professor GRANT, when on my road home from Elchies, sending him these observations, and saying that the star was new to me, and foreign to the "Cycle," but so brilliant and richly coloured, that it could hardly have escaped all former observers. But then it was, that the Scottish historian of astronomy soon identified the star for me with STRUVE'S 2841; and assisted in establishing, in as far as two observations alone can establish, that an Elchies measure whether in magnitude, colour, position, or distance, is not unworthy to stand side by side with an equally small fragment of the noble work of the great Russian astronomer, the maker of Dorpat's fame, and the founder of Imperial Pulkova.

PART C.—*General Deductions.*

PART C.—1. *Instrumental Qualities.*

When the recorded particulars, of what was done with the Elchies telescope during my period of trying it, extend through so many pages as the preceding, a few words on the final results of the whole may not be out of place.

Now, firstly, and most conspicuously, it was by general consent a big telescope,—viz. with an object-glass of 11 inches in diameter. Was it found, then, to possess optical advantages commensurate with that size?

This is an important question to many parties; for mere size, without other advantageous qualities, will only prove a drawback, and of a very positive and incommodious character in the use of any instrument; and while size and its expected power form the chief point upon which the general public desires to be informed, it is also one where amateurs are often exceedingly sensitive; for many a man who has in his day provided himself with one of the largest object-glasses of that period, is not particularly delighted when a dear friend, only a few years afterwards, profiting by the manufacturing progress of the age meanwhile, is enabled with ease greatly to exceed him.

It has recently been said too, by a gentleman who has had possession for many years past of an 8·5-inch object-glass, and with reference to this present testing of the 11-inch telescope of Elchies, that you can always,—without any looking through at the heavens, and by merely measuring with a carpenter's rule the area of any big object-glass,—compute quite near enough what is the smallest isolated star it will show, if its composition be but decently good; that actual astronomical observations to such an end, are therefore idle and useless; and

finally that, as to the small stars picked up by the Elchies instrument, in a trial of that kind, round about 312 Pegasi, No. 27 of our list, he, the objector, had since quite rapidly identified them all with his 8·5-inch glass.

The mere measure, however, of the area in inches does not satisfy every one thus easily, as to each of the many varied star-showing properties or failings of a large object-glass: and the identification quoted above, leaves perfectly untouched two most important data in the problem, viz., 1*st*, The different quality of the two observers' eyes in the two cases, and they may be most extraordinarily different in vigour, sharpness, sensibility or other important optical bearings; and, 2*d*, The fact that the one observer discovered and micrometrically measured the objects, while the other merely identified them from these measures when printed, and placed in his hand. Much is also due on every occasional trial, to the light-transmitting and defining power of the atmosphere at the place; whence, a long series of observations, giving a general mean, should be more convincing than some one special and particular attempt.

Now in the "Cycle," there is a grand mass of evidence of what a good eye was enabled to do, on the average of many years, in an English atmosphere, with a 5·9-inch object-glass; that book, therefore, I make my honoured point of reference in the three following cases:—

First, with a 3·6-inch object-glass I have worked for many nights at the "Cycle" objects in the clear atmosphere of Mount Guajara, but could not, with all my pains, and with all the advantages of that pure and well-defining atmosphere, see all, or by any means all, that the Cycledescribed.

Second, with a 7·25-inch object-glass, I have similarly worked away at the same subjects, and saw without difficulty, anomalies excepted, every feature described in the Cycle; but I hardly saw anything more.

Third, with the Elchies 11-inch, in the bad Elchies atmosphere, I similarly saw, not only without difficulty, but with ease, whatever, even most minute, was described in the Cycle; and beyond that saw and measured, in not a few cases, (I do not say that they were well defined or adapted for making good measures,) other small stars that had neither been described in the respected pages of the Cycle, nor seen with the 7·25-inch object-glass in the purer climate of Alta Vista.

Here therefore is proved, and in an intensely practical manner, the advantage of size generally; also, that together with its superior dimensions, the great Elchies Telescope really has increased optical power proportioned worthily thereto, for distinguishing small isolated stars, whether their existence has been already recorded or not; while the pleasure and satisfaction its use on that account alone imparted to me, is what only an ardent, and previously often foiled, observer can fully appreciate. What would such an instrument not have done in absolute stellar discovery, if the sharper eye of him with the 8·5-inch object-glass, had been employed in utilizing its admirable manifestations!

The next question will be, as to the defining power of the telescope.

This I believe to be good, though the atmosphere was almost invariably so disturbed, that I never could try any very severe test. On one, however, of the only two occasions on which there was good atmospheric definition, I was looking at γ Andromedæ, and had most admirable proof that the smallness of the discs of the stars was just as it should be, with an excellent quality of object-glass of the given size; for, while the Cycle 5·9-inch just barely notched the oblong spot of light formed by the two close stars B and C; and while the Pattinson 7·25-inch on Teneriffe showed them separate, with a black line between them, but yet with their discs mutually compressed on the adjacent sides, and without which compression they must have touched,—the Elchies 11-inch showed them completely separated, and perfectly round; the distance apart of the two stars being about 0″·6, and being believed to have been constant through the three observations.

To these optical advantages, depending on size accompanied by excellence, the Elchies telescope also realized, and showed that there is great satisfaction to the observer in fully experiencing the mechanical advantages of greater steadiness of motion, freedom from tremors, or disturbance by wind, and constancy of zero points,* as necessarily imparted by a large and heavy instrument, than trifled with by all the smaller and lighter ones ever used *by the same observer*.

Part C.—(2.) *Cosmical Results.*

The series of these Elchies observations is too small to expect much from them of this order in the present very advanced condition of sidereal astronomy; but still I do trust that faithful labour with so powerful an instrument has not gone altogether unrewarded.

Beginning with the first of our four telescopic observed quantities, viz., *Mag-*

* The instances in Correction-Table II., p. 377, having been further examined for the often reputed effect of absolute position angle in biasing more or less the estimation of any observer in both position and distance, have given some indications of a small correction of that nature, varying with the position, being required as below; the distances having been first corrected for a probable residual error, both of run of micrometer and method of measuring, amounting at a distance of 5″ to +·04″, at 10″ to +·01″, at 15″ to −·03″, at 20″ to −·08″, and at 25″ to −·13″.

Position Angle.	Correction in Position.	Correction in Distance.	Position Angle.	Correction in Position.	Correction in Distance.	Position Angle.	Correction in Position.	Correction in Distance.
°	′	″	°	′	″	°	′	″
0	−13	+·07	120	+4	−·01	240	0	−·03
20	− 9	+·07	140	+4	−·02	260	− 3	−·02
40	− 4	+·06	160	+3	−·03	280	− 6	·00
60	0	+·04	180	+3	−·04	300	−10	+·02
80	+ 2	+·03	200	+2	−·04	320	−13	+·04
100	+ 3	+·01	220	+1	−·03	340	−15	+·06
120	+ 4	−·01	240	0	−·03	360	−13	+·07

These corrections, though regular, are so small, compared to the ordinary error of individual observations, that they have not yet been applied in any case.

nitude, No. 18, δ Aquilæ, discovers a change of magnitude in its B; No. 7, 222 Arietis, if not the same in its A, at least rectifies a standard book on a test star; and the new small stars found around Nos. 3, 17, 20, 21, 24, 25, and 27, promise to form useful points of reference, touching proper-motion determinations by equatorial observers, such as are much required towards the full understanding, or for the due classification of those stars, according to *character*.

In *Colour* something more has been done; for variations of colour in several stars have been shown to be real and extensive, sometimes affecting both members, and sometimes one only, of a double-star; and in No. 13, the changes are regularly periodical. This case is new, and, if true also, must be of extraordinary importance even in days of "spectrum analysis." It is new, for putting on one side the changing colours of those strange isolations, the temporary stars, and certain degrees of change in tint of stars variable in brightness at the time that they are varying,—the best astronomical writers, from Sir JOHN HERSCHEL downwards, seem to allude to the colours of stars as so many "statical" facts; things to be once well determined by eye or by spectrum, and then stored away in classified lists, or printed in books for permanent reference like the colours of minerals or precious stones. But with our No. 13, or 95 Herculis, its two stars are neither temporary nor variable in magnitude, and yet they change and change exceedingly in colour; their contrasted colours are not the effect of the light of one powerful star dominating that of the other to the eye as a complementary tint, —because the two stars are almost exactly of equal brightness; and again the bluer colour of one is not an effect of greater distance, because the stars are proved, and now proved for the first time, to be binary. Hence, take them all in all, the two stars of 95 Herculis, do seem to be really periodical changers of cosmical colour from causes inherent in themselves, or connected with their own region of space; appearing thereby to indicate the propriety, both of a date being attached to every future observation of star-colour when made, and, on the strength of the analogy between all true stellar orbs, a record of spectrum analyses of the light of our own sun being kept up regularly, as a check upon periodical or secular variations in the quality or material of his light.*

* Some further inquiries may also be subserved by the Elchies colour observations; which, if unusually few in number, have been more than usually attended to touching the character of the double stars concerned, or the difference of the distance of their members from our system.

There is undoubtedly an ether filling space, say most scientific men; well then, if so, what is its colour by transmitted light? Star observations are peculiarly adapted to this end, and the colours which we recognise in all stars may partly belong to this medium, whose colour, too, and composition may be varied in particular regions. First, then, let us ascertain if there is any constant feature of colour dependent on distance. Now the two nearest well-determined stellar systems are those of α Centauri and 61 Cygni; and Sir J. HERSCHEL has remarked their strong yellow colour, stronger in the small, than the large, component in either case; if, then, those stars are really white, but appear yellow to us, they give in so far a quadruple proof of the medium which extends between them and ourselves, being yellow by transmitted light; by no means an extraordinary result, if, according to recent mathematicians, the atmosphere of the earth thins away and extends indefinitely into the planetary and stellar spaces.

In *Position* and *Distance*, the tabular results of observations for each star-group so often giving a mean between distinguished but conflicting observers, as in Nos. 1, 35 Piscium; 4, ζ Piscium; 11, α Herculis; and some others, will prove that useful results have been recorded; though their degree of merit will only be capable of being fully judged at some future day, when impartial computers may, or may not, employ them in orbital calculations. And finally in *Character*, much is believed to have been cleared up for many of the stars, either by the above measures, or the "Proper Motion" test as now applied; in some instances too with singular success, of which Nos. 13, 95 Herculis; 21, ζ Sagittæ; and 22, α Aquilæ, may serve for example.

In conclusion, then, I trust that these pages have now shown that the Elchies

But when we pursue our inquiries further still, and beyond the limits of sensible stellar parallax, and merely employ the broad assumption that the smaller that stars do appear, the further off they must be on the whole,—then arises the anti-terrestrial result, that the greater the depth of the distant medium through which stars are seen, the bluer they become; for all very small stars are bluish. The smaller members also of most double stars, whether optical or physical, and of unknown distance from us, are, as a whole, bluer than the larger members; appearing thereby to imply, though there may be other concurring causes to be mentioned, that the medium filling the more distant realms of space is in a positively different physical condition from that in which we are moving at present under the control and guardianship of our sun.

It is even possible that there are more restricted regions of speciality still, and that floating cosmical motes may exist in interstellar space, capable of playing very peculiar parts on the light transmitted through them from more distant bodies. Cosmical clouds have been suggested by both the Herschels to explain observed changes in telescopic stars and nebula; and such clouds, not very far too from our own system, may be strongly suspected, some might even be inclined to say are found, to exist, from the many variable stars whose periods have been ascertained to be almost exactly 365 days. (See *Spec. Hart.*, p. 269.)

On the other hand, our case of 95 Herculis, even if there had not been any thing else of the same order, establishes the p[illegible]t, of colours and changes of colours peculiar to, or arising in, the stars themselves. That instance i[illegible]ms an extreme case; but the forces at work there are probably in existence elsewhere as well, and not improbably in our own sun, and even in our own earth. Let me explain the hasty generalisation, which, in the absence of almost anything else, may at least incite to further observation. The changing tints of the members of 95 Herculis are eminently auroral, *i.e.* of the character of an Aurora Borealis when its displays are intense, such as we see them in the northern sky once in half a century only, and when fiery pink and vivid green streamers alarm the country; and they are auroral also in their indications of fitful flashes and pulses, like the changes of lustre in periods of a few seconds noticed by Mr Norman Pogson in the star U Geminorum (*Spec. Hart.* p. 107). Again, the pink prominences seen at total eclipses around our own sun, and at times or in parts varying to other bluish colours, as established by Otto Struve, with the approbation of Mr Airy in 1860, have likewise been analogised by Mr Balfour Stuart to terrestrial auroras. Something then of the same character that produces the immense effects observed on the orbs of 95 Herculis, may, and apparently does, exist on the surface of our own sun; following too, in its luminous and coloured manifestations, such active and impetuous changes of short period (for the eclipse red prominences have never been seen twice alike), that some notable maxima, from the occurrence of secular periods, capable of reacting on the climate of our earth, may at times be looked for.

Thus the subject of the colours of the stars becomes one, not only of great astronomical, but also of terrestrial, interest and extraordinary complication; and it is possible that spectrum analysis combined with eye estimations, may enable us to separate the colour produced by transmission through intervening cosmical clouds from the colour of the star itself, by reason of the small difference in the dark lines of the spectrum effected by any colouring material at a low temperature.

telescope not only has remarkable powers for accurate stellar observation, but has effectually made some small contribution to the furtherance of our knowledge of the heavens; and also, that Mr J. W. GRANT of Elchies,—in conceiving the idea, ordering the execution of, and then erecting that fine instrument in the north of Scotland, on a scale so greatly in advance of whatever had been done in the country up to his own, and even the present time,—deserves extremely well of the members of the Royal Society of Edinburgh, and of all scientific men in the land; whose only regret must be, and their sympathy will accompany their regret, that the failure, and nothing but the failure, at the last moment, of Mr GRANT'S long Indian-tried health, prevented him from being the first to use his own telescope, and to communicate possibly important discoveries for the promotion of science.

Page

ART. XXII.—*On a Pre-Brachial Stage in the Development of Comatula, and its importance in relation to certain Aberrant Forms of Extinct Crinoids.* By Professor ALLMAN. (Plate XIII.), ... 241

XXIII.—*Some Account of the Recent Progress of Sanskrit Studies.* By J. MUIR, Esq., D.C.L., LL.D., ... 253

XXIV.—*On Fagnani's Theorem.* By H. F. TALBOT, Esq., ... 285

XXV.—*On the Influence of Weather upon Disease and Mortality.* By R. E. SCORESBY-JACKSON. M.D., F.R.S.E., F.R.C.P., Lecturer on Materia Medica and Therapeutics at Surgeons' Hall, Edinburgh. (Plates XIV.–XVIII.) . . 299

XXVI.—*On the Anatomical Type of Structure of the Human Umbilical Cord and Placenta.* By J. Y. SIMPSON, M.D., Professor of Medicine and Midwifery in the University of Edinburgh, ... 319

XXVII.—*On Earth-Currents during Magnetic Calms, and their connection with Magnetic Changes.* By BALFOUR STEWART, M.A., F.R.S., ... 355

XXVIII.—*On the Great Refracting Telescope at Elchies, in Morayshire, and its Powers in Sidereal Observation.* By Professor C. PIAZZI SMYTH, ... 371

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